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MORE ESPECIALLY OF

## *MINERALOGY, GEOLOGY,*

AND THE

## OTHER BRANCHES OF NATURAL HISTORY;

INCLUDING ALSO

## AGRICULTURE

AND THE

## ORNAMENTAL AS WELL AS USEFUL

## ARTS.

CONDUCTED BY

*BENJAMIN SILLIMAN,*

PROFESSOR OF CHEMISTRY, MINERALOGY, ETC IN YALE COLLEGE, AUTHOR OF TRAVELS IN ENGLAND, SCOTLAND, AND HOLLAND, ETC

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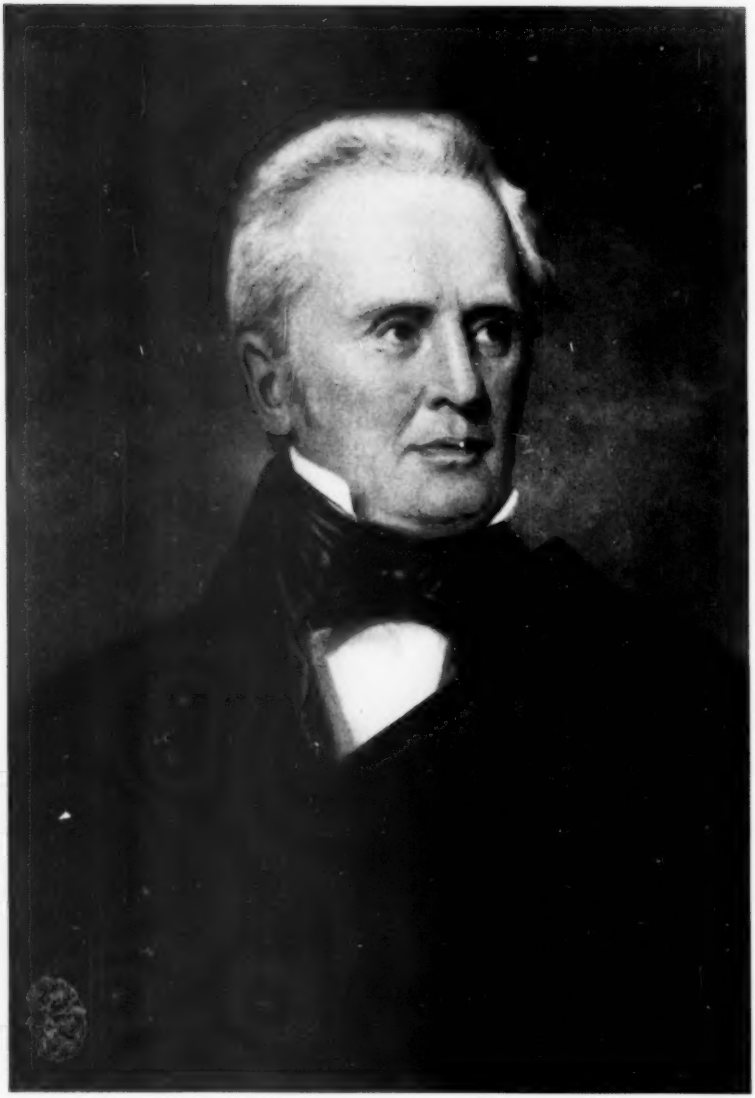
### Reference


Lane, B. H., 1935. Suggestions to authors of papers submitted for publication by the United States Geological Survey: U. S. Geol. Survey, Washington, D. C. (For sale by Superintendent of Documents, Washington, D. C., 25c.)

*ONE hundred and fifty years ago, the founder of this Journal, Benjamin Silliman, began his long and useful career as Professor of Chemistry and Natural History in Yale College. Earth Science in America owes much to his able and vigorous leadership.*

*The cover of this issue bears a facsimile of the cover used by Silliman on early issues of the Journal.*



Benjamin Silliman—1779-1864

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American Journal of Science

*DECEMBER 1952*

# TURBIDITY CURRENTS AND SUBMARINE SLUMPS, AND THE 1929 GRAND BANKS EARTHQUAKE*

BRUCE C. HEEZEN AND MAURICE EWING

ABSTRACT. Following the 1929 Grand Banks earthquake which shook the continental slope south of Newfoundland, all the submarine telegraph cables lying downslope (south) of the epicentral area were broken in sequence from north to south. All previously published explanations of these breaks are considered and rejected because they do not adequately explain this sequence. A new explanation is offered according to which each successive cable was broken by a turbidity current originating as a slump on the continental slope in the epicentral area and traveling downward across the continental slope, continental rise, and ocean basin floor and continuing far out on the abyssal plain well over 450 miles from the continental shelf. We may consider these events as a full scale experiment in erosion, transportation and deposition of marine sediments by a turbidity current in which the submarine telegraph cables served to measure its progress, give evidence of its force, and by their subsequent burial indicate some of the areas of deposition. On the basis of widespread evidence for exposure of Tertiary and older sediments on steep submarine slopes, for numerous coarse graded deposits interbedded with deep sea clays in flat-bottomed ocean basins hundreds of miles from land, we conclude that large scale work by slump-generated turbidity currents is a fundamental process in submarine geology.

## INTRODUCTION

STIMULATED by Daly's (1936) hypothesis that density (turbidity) currents carved the submarine canyons which dissect the continental margins, Stetson and Smith (1937), Kuenen (1937, 1947, 1948, 1950) and Bell (1942) conducted tank experiments from which they concluded that turbidity currents are not only possible in the modern sea but are important agents of transportation. Turbidity currents in Swiss lakes were early described by Forel (1885), and more recently Grover and Howard (1938) described similar currents in Lake Meade and Elephant Butte Reservoir. No matter how convinc-

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ing the experiments and the data from the lakes, they give no definite proof that turbidity currents occur in the modern oceans, although they make it extremely probable. The breakage of the submarine cables following the 1929 Grand Banks earthquake supplies not only an excellent example of a recent large-scale turbidity current but illustrates at least one way in which such currents can be generated. Under this interpretation, a relatively severe earthquake on the continental slope set in motion slides and slumps which, with the incorporation of water, were transformed into turbidity currents of high density. These currents converged to form a gigantic turbidity current which swept across the sea floor for well over 350 nautical miles, breaking each succeeding submarine cable. In this paper the phenomena following the 1929 Grand Banks earthquake are briefly described, explanations of the phenomenon given by previous authors outlined, a new explanation offered and several implications discussed.

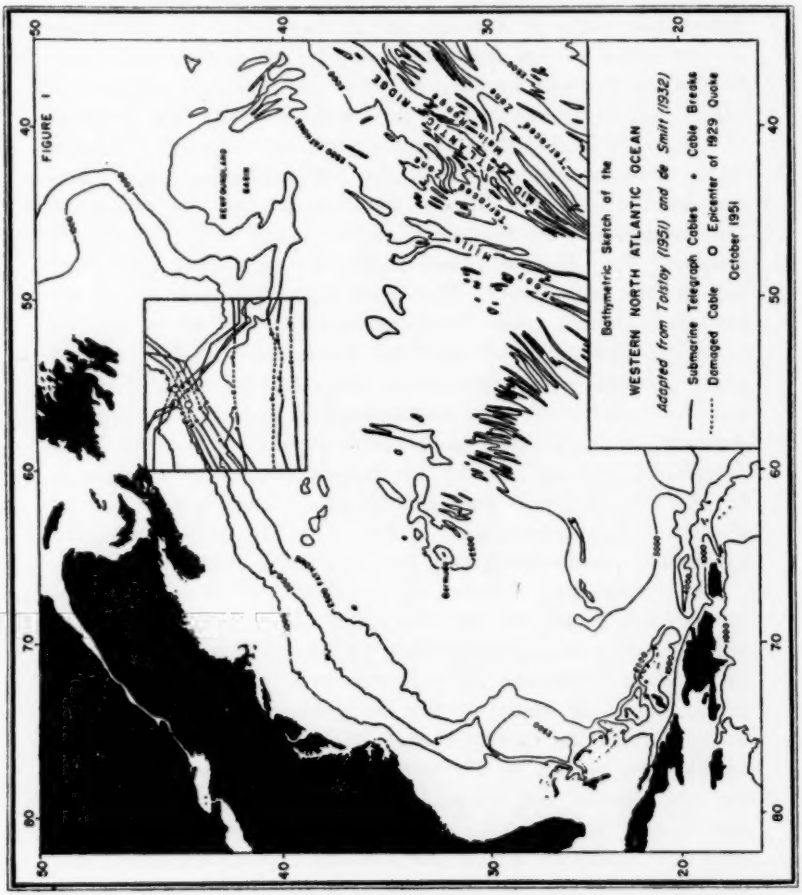
### ACKNOWLEDGMENTS

C. S. Lawton, General Plant Engineer, and V. P. de Smitt, Hydrographer, International Communications Division, Western Union Telegraph Company, gave valuable information and advice and allowed the authors to examine Mr. de Smitt's records of the cable breaks; H. J. Rosenberger, Assistant Chief Engineer, Commerical Cable Company, supplied valuable information from files of his company; and D. B. Ericson, Lamont Geological Observatory, gave valuable information and advice on the sediments. Discussions with W. H. Bucher, Columbia University, and Ph. H. Kuenen, Geologisch Instituut, Groningen, have been extremely helpful. M. T. Flanagan drafted the illustrations.

### SYNOPSIS OF GRAND BANKS EARTHQUAKE

All the submarine telegraph cables from North America to Europe and their Atlantic Coast connections pass south of Newfoundland across either the continental shelf, the continental slope, or the floor of the adjacent deep sea basin. More submarine cables cross this area than any other area of similar size in the world. On November 18, 1929 at about 20 hours

32 minutes G.C.T.[1] a severe earthquake of magnitude 7.2 shook the continental shelf and continental slope south of Newfoundland. The epicenter lay between 1000 and 2000 fathoms on the continental slope southeast of Cabot Trench in the midst of this dense network of cables. The six cables lying between




[1]Hodgson and Doxsee (1930) list the time and epicenter as 20h 31m 55s at Lat. 44.5° N, Long. 55° W. The International Seismological Summary lists 20h 31m 45s at Lat. 44.55°N, Long. 55.95°W and aftershocks with the same epicenter at 23h 0lm 48s and November 19 at 02h 0lm 18s. Gutenberg and Richter (1949) list the time as 20h 31m 58s at Lat. 44° N, Long. 56° W with a magnitude of 7.2.

150 and 1800 fathoms on the continental slope between Long. 54.5 W and 57.5 W were broken instantaneously by the initial ground motion or by the almost instantaneous slides and slumps. For 13 hours 17 minutes following the quake there occurred an orderly sequence (de Smitt, 1932) of breaks of each succeeding cable lying in increasingly deeper water for over 300 miles south of the epicentral area (fig. 1). Although all the cables on the continental slope and on the ocean floor to the south were broken, not one of the large number of cables laid on the continental shelf was disturbed. The instants of the cable interruptions were accurately recorded by automatic machines which record the telegraphic messages, and the locations of the breaks were determined by resistance measurements from the shore ends of the cables. Table 1, adapted from Doxsee (1948), lists the positions, times and depths for each break. The earthquake was felt along the eastern seaboard from Newfoundland to New York, and in Nova Scotia and Newfoundland (Johnstone, 1930) caused slight damage to unmortared chimneys and to unstable slopes near quarries and road embankments, and the spilling of mercury in lighthouses. A tsunami swept up towards Newfoundland, causing considerable loss of life and great damage to the fishing boats and fishing gear along the shores of Placentia Bay (Johnstone, 1930). A seaquake was observed by several vessels navigating over the area (Gregory, 1931).

The relationship between earthquakes and submarine telegraph cable breaks was pointed out by Milne (1897), and the hope of using seismological observations to avoid areas likely to produce numerous cable breaks was responsible for the establishment of many of the early seismograph stations. It was not until the earthquake of 18 November 1929 when 12 cables were broken in 28 places that any intensive study of the phenomenon was made. De Smitt's (1932) study of this earthquake "distinctly shows (1) that the direction of progression of destruction was to the seaward . . . to the south and southeast from the epicenter, that is from the continental slope towards the deeper parts of the ocean, (2) that the velocity of progression decreased with increase in distance from the epicenter, (3) that the area of broken or mutilated cables was restricted to the deepest part of the ocean—narrow in the northern part and broadening to the south, 355 nautical

TABLE 1

List of Cable Breaks after Doxsee (1948)

| | Break No. | Lat. N. | Long. W. | Fathoms Depth | Time (GCT) 11/18/29 | Interval between quake and break | Remarks |
|---|---|---|---|---|---|---|---|
| A | Commercial Cable Company, New York-St. Johns (N. Y. No. 1) | | | | | | |
| | 1 | 44°32′ | 55°04′ | 1450 | 2033 | 0 | These breaks in epicentral area are probably due to landslides and slumps and initial ground movement. |
| | 2 | 43°41′ | 57°07′ | 1890 | ? | ? | |
| B | Commercial Cable Company, Canso-Fayal (Main No. 4) | | | | | | |
| | 3 | 44°23′ | 55°08′ | 1500 | 2033 | 0 | |
| | 4 | 44°43′ | 56°10′ | 220 | 2033 | 0 | |
| C | Imperial Cable Company, Halifax-Harbor Grace | | | | | | |
| | 5 | 44°50′ | 55°09′ | 800 | 2032 | 0 | |
| | 6 | 44°55′ | 54°45′ | 700 | 2033 | 0 | |
| D | Commercial Cable Company, New York-St. Johns (N. Y. No. 2) | | | | | | |
| | 8 | 44°12′ | 55°10′ | 1820 | 2033 | 0 | |
| | 9 | 43°47′ | 57°17′ | 1830 | 2033 | 0 | |
| E | Commercial Cable Company, Canso-Fayal (Main No. 6) | | | | | | |
| | 10 | 44°07′ | 55°18′ | 1780 | 2033 | 0 | |
| | 11 | 44°45′ | 56°09′ | 310 | 2033 | 0 | |
| F | Western Union Cable Company, New York (Hammel)-Bay Roberts (No. 1) | | | | | | |
| | 12 | 44°02′ | 55°02′ | 1800 | 2032½ | 0 | |
| | 13 | 43°25′ | 56°20′ | 2000 | 2033 | 0 | |
| | 14 | 42°42′ | 58°10′ | 2300 | ? | ? | |

TABLE 1 (*Cont.*)

List of Cable Breaks after Doxsee (1948)

| | Break No. | Lat. N. | Long. W. | Fathoms Depth | Time (GCT) 11/18/29 | Interval between quake and break | Remarks |
|---|---|---|---|---|---|---|---|
| G | French Cable Company, Cape Cod-Saint Pierre | | | | | | This cable within the epicentral area held up for 13 minutes before being broken. |
| | 15 | 44°20′ | 56°40′ | 1150 | 2046 | 0014 | |
| | 16 | 44°03′ | 57°12′ | 1000 | ? | ? | |
| H | Imperial Cable Company, Halifax-Fayal | | | | | | The following breaks downslope from the epicentral area are due to turbidity current. |
| | 17 | 43°37′ | 55°15′ | 2000 | 2131 | 0059 | |
| | 18 | 43°15′ | 56°07′ | 2200 | 2131 | 0059 | |
| I | French Cable Company, Cape Cod-Brest | | | | | | |
| | 19 | 42°05′ | 55°30′ | 2400 | 2335 | 0303 | |
| | 20 | 42°07′ | 53°30′ | 2600 | ? | ? | |
| | 21 | 42°00′ | 57°36′ | 2500 | ? | ? | |
| J | French Cable Company, New York-Fayal | | | | | | |
| | 22 | 40°30′ | 55°55′ | 3000? | 0533 | 0901 | |
| | 23 | 40°28′ | 52°06′ | 2800 | 0533 | 0901 | |
| K | Western Union Cable Company, New York (Hammel)-Bay Roberts (No. 2) | | | | | | |
| | 24 | 40°00′ | 55°20′ | 3190? | 0650 | 1018 | |
| | 25 | 40°02′ | 54°50′ | 2900 | 0650 | 1018 | |
| | 26 | 40°13′ | 52°30′ | 2800 | 0850? | 1218? | |
| L | Western Union Cable Company, New York-Fayal (Horta) (No. 1) | | | | | | |
| | 27 | 39°29′ | 53°47′ | 2780 | 0949 | 1317 | |
| | 28 | 39°35′ | 51°41′ | 2800 | 0958 | 1321 | |

miles long and 210 nautical miles wide, with breaks radiating outward and with not a single break on the shallow continental shelf, and (4) that the ocean floor in the neighborhood of the breaks is rather even with an average angle of slope with the horizon of 1°50' and that about one half the breaks were on a slope of less than 1°."

### SUMMARY OF PREVIOUS INTERPRETATIONS

Gregory (1929, 1931), Hodgson and Doxsee (1930), Keith (1930) and de Smitt (1932) attributed the cable breaks to movement along two southeast-trending faults extending along each side of Cabot Strait and subsequently across the continental slope and into the ocean basin. Gregory (1931) concluded that

> "The breakage of cables along both sides of the (Cabot) trench indicates that the movement was a renewed subsidence of the floor between parallel faults, and that the trench is a submarine rift valley . . . as the earthquake was sufficiently powerful to break cables over an area extending 360 (nautical) miles N and S and 150 (nautical) miles E and W, some displacement of the sea floor is probable . . . The clear evidence that Cabot Strait and Trench is a submarine rift valley indicates that the submarine canyons off the Congo and the Hudson and the Adour are probably also tectonic and due to subsidence of the ocean floor along faults."

He assumed that Cabot Strait was a down-faulted block and that the apparent coincidence of cable breaks with projections of these walls proved the extension of the assumed faults. The sequence of the breaks was dismissed with the following statement:

> "These more distant cables either yielded . . . to a stress that had been acting for about thirteen to fourteen hours or they were broken by a subsequent earth movement or slip of the sea floor."

McIntosh (1930) believed that the Cabot Trough was cut by an "old Laurentian river" which carried a "tremendous amount of rock waste from its bed and basin" and deposited it on the continental margin "seaward from its mouth." He attributed the Grand Banks earthquake to "settling and fracturing of the thick mass of sediment concealed beneath the

waters of the embayment." However, he made no attempt at explaining the sequence of breaks.

Johnstone (1930) gave an account of the effects of the quake in Newfoundland and Nova Scotia and mentioned the cable breaks, attributing their breakage to "very great vibratory and oscillatory forces." Neither McIntosh nor Johnstone seems to have been aware of the orderly sequence of the breaks, for neither author mentions it.

Hodgson and Doxsee (1930) concurred with Gregory that the cable breaks were due to the down-faulting of a portion of the sea floor, "the northern end being the more seriously displaced." On the orderly sequence of the breaks they wrote:

> "To account for the breaks occurring at times successively later as one moves south, we may suppose all the cables loaded more or less with silt and ooze, which serves to weigh them and, in a measure, to hold them in place. A drop of the bottom of, say 25 feet, at 44.5° N and considerably less at 39° N left the cables unsupported at two points at least, which caused them to break due to the weight of the silt lying on them. The reason advanced for the southern cables yielding so much later is that the drop in the ocean floor may have been less there."

Keith (1930) concluded that

> "All the evidence is in harmony with the theory that parallel faults produced the Cabot Trench in the past and the Grand Banks earthquake in the present."

He continued:

> "There still remain to be explained the cable breaks far to the southwest off the extensions of the trench margins. By the same line of reasoning other and parallel faults must be appealed to for explanation of these breaks. Direct evidence of this arrangement appears to be given by the difference in time between some of the breaks."

Apparently Keith believed that the difference in the time of the different breaks was due to the difference in the time of the supposed faulting along "secondary" faults.

De Smitt (1932) concurred with Gregory, Hodgson and Doxsee, and Keith in attributing the breaks in the cables to parallel faults. In his opinion "the severely shaken area doubtless extends beyond the southernmost broken cable."

After a study of the Cabot Strait, Shepard (1931) concluded that

> "the trough has been shaped principally by glacial erosion . . . (and that) the Grand Banks earthquake was associated with the irregular topography of the continental slopes and deep ocean basin rather than with the St. Lawrence submarine trough, suggesting that the proximity of the trough may be only coincidental."

Kindle (1931), although accepting faulting as the cause of the breaks in the vicinity of the epicenter, pointed out that faulting explains neither the multiple breaks in the cables near the epicenter nor "the north to south progression of cable breaks, with an interval of 13 hours between the first and last breaks." To explain the burial of some of the cables, Kindle drew an analogy between the Grand Banks earthquake and the New Madrid earthquake of 1811 in which the "soft" ground of the Mississippi bottom land was severely disrupted and quantities of sand were expelled through fissures. He quoted statements by Murray and Hjort and Milne concerning "tidal currents" at great depths in the ocean, and concluded that "we must . . . assume . . . the existence of sea-bottom currents in depths as great as 2,000 fathoms or more." He considered the currents to be due to the combined action of tides and storm winds. The shock of the earthquake was considered to have left the bottom sediments in an unstable condition so that these currents, while normally incapable, could under these special conditions erode the bottom. He concluded that

> "The progressive north-to-south order of the breaks appears to square well with the hypothesis that most of the breaks were caused by submarine landslides developed through the agency of bottom currents which in turn were produced chiefly by the gale blowing at the time of the earthquake."

Shepard (1932, 1934) states that "there is reason to believe that Corsair Gorge was opened (by a landslide) at the time of the Grand Banks earthquake in 1929." Johnson (1939) pointed out the weakness of this statement and the improbability that the supporting evidence is significant. To his objections can be added the fact that the records of the Western Union Company show no breaks in several cables passing through

and near the Corsair Gorge at the time of the Grand Banks earthquake.

Stetson and Smith (1937), in discussing suspension currents and mudslides, have concluded that about 10 grams per liter of typical continental shelf sediment would be required to increase the density of water of the continental shelf in summertime sufficiently to enable it to sink to the foot of the continental slope, whereas about 3 grams per liter would be required in winter. These writers have emphasized that such currents will leave the continental slope and spread out horizontally when they reach a depth at which the density of normal ocean water exceeds that of the suspension. In the paper of Stetson and Smith there is a section on slumping in which the possibility that slumps could be induced by the frictional drag of suspension currents (turbidity currents) is discussed and considered negligible. There is no mention in their paper of the possibility that slumps on the continental slope may be transformed into turbidity currents, thereby enabling the sediment to be carried far out into the ocean over very gentle bottom slopes. It seems clear that they were considering the slumps only from the viewpoint of agents in canyon erosion. Their statement that "Submarine landslides on a major scale are known to occur, as for instance the one which broke the Western Union Company's cables off the Grand Banks of Newfoundland following the earthquake of 1929," referring as it does to the cables of only one company without reference to the literature and without statements about the slopes and distances involved, indicates that no detailed examination of the cable break data was made.

Bucher (1940) offered an explanation which has some points in common with that of Kindle, stating that

> "The picture becomes intelligible when the breaks are interpreted as the result of erosion along the sides and bottoms of submarine valleys brought about by the tsunamic waves set up by the earthquake . . . (The tsunami) were probably created by submarine slides set off by the earthquake and some of the cables were undoubtedly broken and buried by such slides, especially along the faults that bound the graben of Cabot Strait."

He supposed that the cables broke "of their own weight" when the canyons were scoured sufficiently to "leave portions of the

cables unsupported." Since the velocity of the tsunami was several times that of the propagation of the breaks, he assumed that

> "With increased distance from the origin, however, more and more of the energy of the tsunamic waves spreading into still water was lost, and the orbital velocity of the water particles reduced proportionately. The resulting weaker current required more time to produce the same amount of erosion."

Shepard (1948) in part followed Kindle and Bucher, stating:

> "Since the breaks did not follow any line suggesting faulting, it may be assumed that they were due to mass movements of the loose sediment leaving sections of the cable unsupported."

Doxsee (1948) stated:

> "The fact that the breaks range themselves into two groups whose east and west separation becomes greater as the progression extends southeast from the epicentral zone to about 39° north latitude suggests a subsidence of the ocean floor between two fault planes each roughly parallel to the axis of the Cabot trench and extending from about 45° N to approximately 39° N."

The fact that the cables to the south broke later "would indicate that the greatest displacement was in the northern section of the disturbed zone." Doxsee's paper, probably written in 1932 but not published until 1948, expresses the same opinion as Hodgson and Doxsee (1930).

Kuenen (1950) in discussing cable breaks states:

> "In many cases the ruptures are not due to direct faulting of the sea floor but to sliding . . . (1) the surface over which ruptures occur is often very extensive . . . (2) the cables are frequently found buried . . . (3) many breaks may occur several hours after the shock and successively later at greater distances from the epicenter. This indicates a secondary cause, probably tsunamis."

## DISCUSSION OF THE PREVIOUS THEORIES

Although most of the previous authors were aware of the orderly succession of breaks from north to south into successively deeper water, none of the above explanations satisfactorily explains the sequence. Gregory's statement that the

later breaks may have been due to stresses acting for several hours or to later land slips offers no explanation for the orderly succession. Hodgson and Doxsee's explanation is equally unsatisfying. The explanation of Keith, attributing the later breaks to a complex system of later faults, seems excessively complicated and offers no explanation for the later succession. The later faults should be expected to cause later earthquakes. Only two aftershocks were recorded although there were six later cable breaks; further, the times of the aftershocks do not correspond to the times of the later breaks, and the epicenters were approximately identical with those of the original quake and the magnitudes much smaller. Gregory's and de Smitt's impression that the whole area from 45° N to 39° N was severely shaken by the quake resulted from their assumption that all the breaks were directly caused by seismic action. The determinations of the epicenter showed no such trend. The explanation given by Kindle that hydraulic currents set up by a gale were primarily responsible for the breakage and that the only role of the quake was to render unstable the bottom sediments is unsatisfying even if one considered such action by that type of currents to be possible. That a quake would bring sediments to a critical stability over a 350-mile area but that no movement would occur until a hydraulic current passed over the area is almost incredible.

Bucher's idea was that the tsunami which was set up by the earthquake caused erosion in the submarine canyons which the cables passed across, and that when erosion proceeded far enough to leave the cables unsupported over a certain unspecified distance they broke. Further, since he supposed that in the deeper water farther from the quake the erosive action of the tsunami was less, it would take longer for this unspecified amount of undermining to take place. The velocity curve (fig. 3) shows the time of the breaks *versus* distance from the epicentral area. In every case each succeeding point falls near the curve formed by the other points. The cables were of widely different ages, different types of construction and consequently different breaking strengths. Only a close adjustment of degree of undermining and breaking strength would produce so smooth a curve, an adjustment which is highly unlikely. Since the breaking strength of a new cable such as the farthest cable is about 10½ English tons (Higgins in Hodgson and Doxsee,

1930) and its weight per mile about 1.1-1.3 English tons in water, the canyons would have to be widened by several miles. The topography is not very accurately known in the region of the deeper breaks, but it can be said that no canyons of the size required by Bucher's hypothesis occur in those depths. Further, this hypothesis does not explain the concurrent burial of hundreds of miles of the deeper cables.

This criticism applies also to Shepard's idea that the cables broke because mass movements left sections of the cables unsupported. The previous published hypotheses are unsatisfactory for various reasons, but they have in common one major failing—none of them satisfactorily explains the orderly succession of breaks from shallow to deeper water.

### CABLE BREAKS IN GENERAL

Soon after the laying of the first successful Atlantic submarine cable in 1866, it became obvious that there was much more activity beneath the sea than had been anticipated. Benest (1899), in the introduction to a very informative paper, states that "accidents to cables have already been valuable . . . in directing attention to hitherto unsuspected forces constantly in action and altering the features of the sea bottom." Milne (1897) and Benest (1899) presented much valuable evidence concerning the breakage of cables. Although we may not agree with their interpretation of the facts, it will be valuable to cite a few examples given by these early workers. Milne mentions one case which may have been similar to the occurrence after the Grand Banks earthquake. Speaking of the October 4, 1884 breakage of three cables running parallel to the contours on the southeast side of the Grand Banks in about 1500 fathoms depth, he states:

> "A very significant fact is the case when three cables running in parallel lines 10 miles apart broke at points nearly opposite to each other, on the same straight line."

The records are not clear but there appears to have been some delay in breaking from cable to cable, indicating that the cause of the breaks could have been a slump or turbidity current. Milne mentions numerous examples of cable breaks in submarine canyons, and repeatedly in his notes on the cause of the breaks the statement "landslides or earthquakes" appears.

Benest (1899) discusses repeated breaks of two different cables in a submarine canyon north of Cape Verde, West Africa. The breakage was finally avoided by laying the cables on the continental shelf. He also gives an account of the repeated breakage of the Central and South America Cable Company's cable off Talara, Peru in April 1891, March 1892 and April 1892. He states on the authority of the captain of the cable ship that

> "It was noticed in every or nearly every case that the specimen of bottom brought up from the deep part of the gully was coarse grey sand and small stones: that of the sides, a very tenacious clay; and from the comparatively level part farther away, soft green mud."

This description squares well with the condition found to prevail in the Hudson Canyon (Ericson, Ewing and Heezen, 1951) in which turbidity current transportation of the coarse sediment is unquestioned. It may also mean that the walls of this South American canyon, like the Hudson, are composed of Tertiary sediment. To avoid breakage Benest states that on several other occasions the cables have been relaid inshore at a higher level,

> "notably off the Rovuma River, in the cable between Zanzibar and Mozambique . . . (which) broke down eight years in succession. Since it has been relaid inshore, some twelve years ago, it has never broken down . . . (After) the São Thomé Landa cable . . . had broken down twice in fifteen months, it was relaid nearer the shore, and it then lasted five or six years."

Here again is an instance when those cables which passed along the slope through the canyons were repeatedly broken, but when relaid inshore they were not disturbed, just as the many cables on the Grand Banks were not disturbed after the Grand Banks earthquake. De Smitt (1932) made a study of the correlation of cable breaks and earthquakes for all the pre-1930 breaks. This report contains much valuable information on cable breaks.

It can be concluded from a study of the published reports that cable breaks are very frequent on the continental slopes, especially in canyons. It is true, however, that the greatest number of breaks occur on the continental shelf, but most

of these can be explained by abrasion to shallow water near shore or by snagging by trawlers, whereas the slope breaks have up to now been harder to explain.

### PRESENT KNOWLEDGE OF TURBIDITY CURRENTS

The present authors, together with Tolstoy, conducted a survey of the seaward extension of the Hudson Canyon in 1949 (Tolstoy, 1951; Ericson, Ewing and Heezen, 1951; Heezen, Ewing and Ericson, 1951; Heezen, Ewing and Tolstoy, in preparation). The striking streamlike characteristics of the canyon, which were so well described in the works of Veatch and Smith (1939) for the portion of the canyon which dissects the continental slope, were found to persist in the previously unknown portions which extend for another 200 miles from the foot of the continental slope across the continental rise to the floor of the abyssal plain at 2650 fathoms. The sediments strongly indicated that turbidity currents had been active in the canyons but not in the intercanyon areas of the continental rise. Sands were found over a wide area near the end of the canyon on the abyssal plain and the base of the continental rise. Gravels and sands were found in the bottom of the canyon overlying Tertiary sediments, and outcrops of Tertiary sediment were revealed on the canyon walls, while the intercanyon divides of the continental rise were covered by "normal blue silty mud." In a discussion of Tolstoy's paper Heezen, Ewing and Ericson (1951) suggested that the abyssal plains of the ocean basins were formed by the ponding of sediments brought in by turbidity currents. Ericson, Ewing and Heezen (1952) offered criteria for recognition of sediments transported to great distances by turbidity currents, and evidence of transport to distances of the order of 1000 miles.

A number of other canyons between Cape Hatteras and Flemish Cap that have recently been sounded and sampled for sediments in rapid reconnaissance fashion appear to have the same characteristics as the Hudson Canyon. A large canyon a few miles north of Great Abaco Island of the Bahamas, and also the Northeast and Northwest Providence Channels and the Tongue of the Ocean Canyon in the Bahama Islands, have been surveyed and sampled in a reconnaissance fashion. Here also are found streamlike topography, coarse clastics on

the canyon floor, older sediments exposed on the walls and a great delta composed of numerous graded beds of calcareous sediment. The authors, together with Hersey, have conducted reconnaissance seismic, topographic and sediment surveys in the Puerto Rico Trench north of Puerto Rico. The deepest part of the trench was found to be flat at about 4700 fathoms (corrected to true depth) and this flat floor was found by coring to be underlain by a succession of graded beds of calcareous sediment and thin layers of the red clay "normal" for that depth. The flat floor and the graded "shallow water" sediment are undoubtedly the result of turbidity current deposition.

Studies of topography, sediment cores, and seismic refractions within about 60 miles of Bermuda have demonstrated that surrounding the volcanic core of the island there is a great fan of calcareous sediment containing numerous graded beds of shallow-water material. In moving down the slope to deep water, the turbidity currents which undoubtedly deposited these graded beds eroded the flanks of the Bermuda pedestal and exposed Tertiary sediments (Ericson, Ewing and Heezen, 1952).

The outcrops of Tertiary sediment reported from the continental slopes of the world are compelling evidence for present-day erosion of the sea bottom by turbidity currents (Northrop and Heezen, 1951; Stetson, 1949; Shepard, 1948). The graded beds prove that the erosive agent acts intermittently and the gravel found in the canyons (Ericson, Ewing and Heezen, 1951, 1952) proves that its transporting capacity must be great, therefore its velocity must be great. Thus there is strong evidence that turbidity currents are a major factor in submarine geology. Additional support for the concept that turbidity currents are a fundamental submarine process is given in a recently published symposium on turbidity currents (Symposium, 1951).

## EXPLANATION OF THE CABLE BREAKS BY TURBIDITY CURRENTS

The mounting evidence for the vast importance of turbidity currents and slumps in the sea and the inadequacy of all the published hypotheses to account for the regular sequence of the cable breaks led the authors to consider the following explanation. A severe shock jarred the continental slope and

shelf, setting landslides and slumps in motion. These virtually instantaneous movements affected an area 80 by 150 miles along the continental slope. These mass movements, starting on the relatively steep continental slope, raced downward, and by the incorporation of water the moving sediment was transformed from sliding masses into turbidity currents. Undoubtedly the pattern of canyons and tributaries caused initial concentrations of the flow, but as the canyons joined the currents grew larger, quickly becoming so large that they were not restricted by the submarine canyons and eventually covered the bottom of the 200-mile-wide bight which lies between the southern Grand Banks and the continental slope off Sable Island. The currents had many times the force necessary to break the cables, and snapped each cable shortly after reaching it. In all cases, at least two breaks occurred 100 miles or more apart, and the intervening section of cable was either buried or carried seaward so far that it was never found. As the current began to slow down, because of decreasing slope, finer and finer sediments were deposited in a manner which produced graded bedding. Since the flow passed the last cable in the area at a distance of about 400 miles from its origin with a velocity of 12 knots and sufficient force to destroy 200 miles of the cable, it is probable that the area of deposition extended hundreds of miles farther.

Mr. C. S. Lawton and Mr. V. P. de Smitt, after hearing the writers' interpretation, brought from the files of the Western Union Company a letter written by A. C. Veatch to G. H. Ridge, then Ocean Cable Engineer of the Western Union Company. In this unpublished letter of December 13, 1934, Veatch emphasized the significance of several facts in de Smitt's unpublished report (1932). He noted as significant the sequence of breaks, the fact that none of the cables on the continental shelf were disturbed, that the cables on the steep continental slope were not buried in the least while the deeper cables were buried extensively. He stated that

> "All these facts, together with the detailed data that have been collected on some earthquakes, notably the great San Francisco earthquake, point to the conclusion that the breakage of the cables was due, not to the relatively minor displacement which caused the earthquake, but to one or more landslips produced by the earthquake."

He concluded that the orderly sequence of cable breaks proved that the principal agent responsible for the cable breaks was a single major landslide affecting an area whose dimensions are measured in hundreds of miles and that this extensive landslide originated near the epicenter on the steep continental slope and came to rest near the last cable. This view is similar to that of the present authors, the main difference being that Veatch did not suggest any mixing of the sediment and the sea water; therefore he was led to the view that the disturbance was a landslide rather than a turbidity current such as the present authors have described.

The transformation of the landslide into a turbidity current explains two major points. The first of these is the graded nature of the sediments deposited—a fact unknown to Veatch. The second is that a landslide could hardly carry on to so great a distance over so slight a gradient. Veatch attempted to make this plausible by appeal to the lubricating action of the deep-sea clays, but in the opinion of the present authors this point alone is a fatal objection to the simple "landslide" theory. Veatch believed that this landslide could not possibly have gone much farther seaward than the southernmost cable and therefore suggested that future cables be laid farther to the south to avoid damage, a conclusion with which, as will be seen later, the present authors do not fully agree. Shepard also suggested that landslides were responsible for the breakage of the cables; however, he suggested that segments of the cable were broken when undermined by local slides, while Veatch suggested that the breaks were due to the force exerted on the cables by sliding masses of sediment.

The topographic profile with superimposed velocity graph (fig. 2) clearly shows the positive relation between velocity and slope. The velocities shown on figure 2 were determined by drawing tangents to the time-distance curve (fig. 3), and are subject to the inaccuracies inherent in this method. The horizontal scale for the profile is the measured distance along the axis of the bight extending south from Cabot Strait. A better knowledge of the topography of the area may alter the distances and change the velocities to some extent. It can be seen that on the upper portion of the continental rise where the slope is about 0.5 per cent the velocity is about 50 knots, while on the very gently-sloping oceanic floor to the south

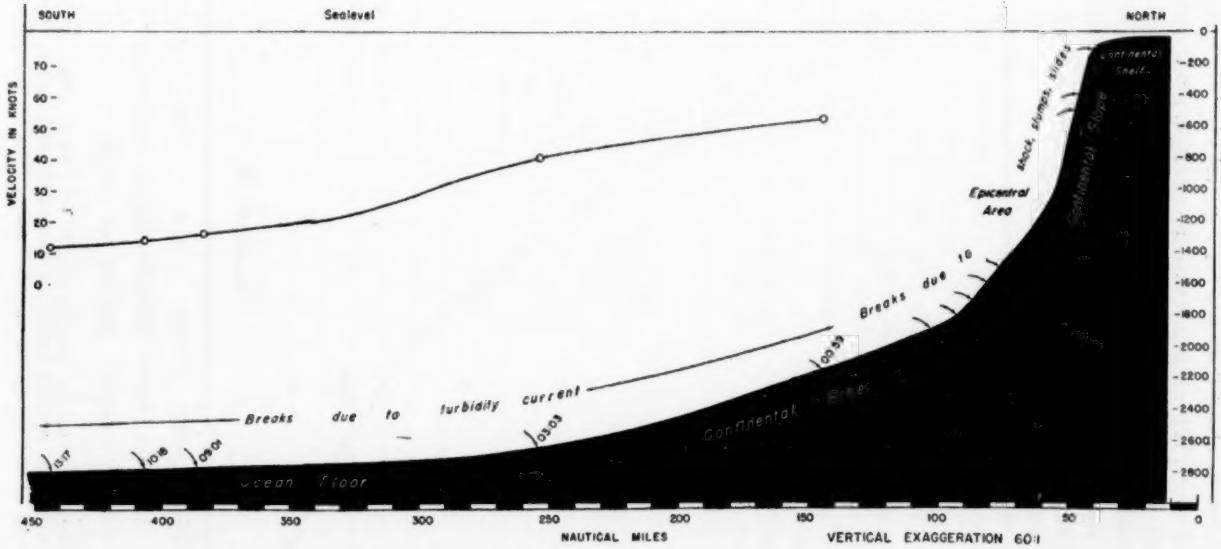



**Fig. 2. Topographic profile south of Cabot Strait with superimposed graph of the velocity of the turbidity current as determined by the successive cable breaks.**

where the slope is about 0.05 per cent the velocity has dropped to about 12 knots. With this decrease in velocity a great amount of material must have been deposited, as indicated by the extensive burial of the deeper cables.

On the theory that a turbidity current should produce a prolonged disturbance at the ocean surface, the tide records

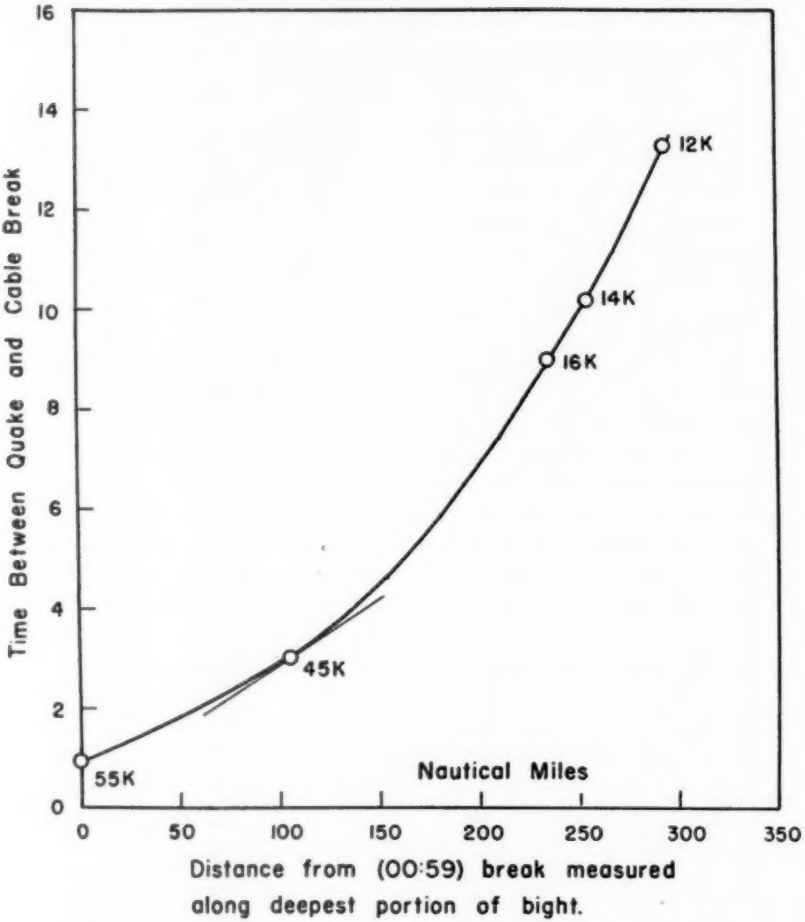



Fig. 3. Distance along the deepest portion of "bight" south of Newfoundland vs. interval between instant of earthquake and instant of cable break. Time in hours, distance in nautical miles, velocity in knots (K).

obtained at Halifax, Nova Scotia, and Atlantic City, New Jersey, following the Grand Banks earthquake, were examined. Each showed a train of waves associated with the tsunami which may be related to the passage of the turbidity current. The waves had periods of one to two hours and heights of about one foot, and they continued until about 1500 G.C.T. 19 November 1929, about 20 hours after the seismic shock and about 5 hours after the last cable break. The possibility that this tidal disturbance was due to the heavy gale blowing south of Newfoundland at the time has not been entirely removed, but strong tidal surges at Bermuda and the Azores (Doxsee, 1948) following this earthquake indicate that the disturbance of the Halifax and Atlantic City tide gauges was probably not due to the strong gale. A study of tide-gauge records from all parts of the Atlantic for this and for other cable breaks is being undertaken, and it is hoped that through this study criteria may be developed to identify the passage of turbidity currents in the past and in the future by the study of mareograms.

Evidence of numerous large turbidity currents, probably starting as submarine slumps, is found in the study of deep-sea sediments (Ericson, Ewing and Heezen, 1952). Such evidence, together with that presented in the present paper, offers strong support for the view held by many seismologists (see, for instance, Gutenberg, 1939) that the principal cause of large tsunamis is submarine slumping. In this view the correlation between T phase production and tsunami production (Ewing, Tolstoy, Press, 1950, and Ewing, Press, Worzel, 1952) simply means that the situation which is favorable for efficient transmission of horizontally traveling compressional waves from the solid earth into the water is equally favorable for generation of large submarine landslides. Both phenomena depend upon strong earthquake waves impinging on a steeply sloping ocean bottom. The two principal factors are availability of material in an unstable position and the earthquake which triggers it into a large scale landslide. If the unstable material is sediment slowly accumulated, the frequency of large earthquakes in the area may determine the size of the tsunami, but if the unstable topography is due to present orogeny there may be no relation of tsunami size to earthquake frequency.

The type of turbidity current which affected the area south

of the Grand Banks was obviously in some way different from the turbidity currents which have carried sediment down the Hudson Canyon. In the case of the Hudson the evidence is strong that the current was largely restricted to the canyon until at a depth of about 2400 fathoms the sands were deposited by distributaries. In the case of the Grand Banks the current flowed over a segment of the sea floor 150 miles wide. The difference is interpreted as largely one of scale. Probably none of the deposits which we now see associated with the Hudson Canyon were deposited by as extensive a current as that following the Grand Banks earthquake, as shown principally by the presence of undisturbed sediments between canyons.

Daly supposed that turbidity currents were generated by the turbulent action of storm waves on the continental shelf. Here we suggest that the Grand Banks earthquake triggered off a very large turbidity current. Although convinced of the great importance of the earthquake triggering effect, the authors do not wish to imply that it is an exclusive process. Turbidity currents probably are generated in a number of ways. Storm waves, tsunamis, and internal waves may cause mixing of sediment with the sea water as suggested by Daly (1936), or these processes, plus earthquakes, may cause slumps and slides which may with the incorporation of quantities of sea water be transformed into turbidity currents. It seems likely that the most powerful (high density) turbidity currents are more frequently formed by the transformation of slides and slumps.

Whether one considers the phenomenon which followed the Grand Banks earthquake a landslide, a mud flow, a turbidity current, or a combination of the three, its importance as an agent of suboceanic transportation is great. The suggestion was made by Heezen, Ewing and Ericson (1951) that the flat floors of the ocean basins and oceanic trenches are due to the ponding of sediment derived from turbidity currents or similar bottom-seeking movements. The large scale movement after the Grand Banks earthquake serves as overwhelming proof that currents do occur which will transport large amounts of sediment far out into the oceanic basin. The southernmost cable was on the floor of one of the great oceanic plains described by Tolstoy (1951) and Tolstoy and Ewing (1949). It will be remembered that the velocity of the current as it passed the last cable was of the order of 12 knots, and that the

sediment dredged up by the cable ships making the repairs contained "sharp sand and small pebbles." A formidable quantity of sediment must have been carried for hundreds of miles to the south across the gently sloping deep sea plain as the current lost velocity and deposited the rest of its load, which undoubtedly contained vast quantities of mud and silt. Successive turbidity currents should create a deep sea sediment which contains many alternations in lithology from sand to mud. Each such current would travel a slightly different path as the flows originated at different points along the continental slope and spread overlapping blankets of sediment over the deep sea floor.

Although it is impossible to draw a line separating an area of erosion from one of deposition in the present case, the fact that several submarine canyons have been shown to continue to comparable distances from shallow water with sands and gravels concentrated in their beds leads to the conclusion that the events associated with the Grand Banks earthquake of 18 November 1929 may be considered as a full-scale experiment in erosion, transportation, and deposition of marine sediments by a turbidity current in which the submarine telegraph cables served to measure its progress and to give tangible evidence of its force.

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Lamont Geological Observatory
Palisades, New York

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# ESTIMATED SIZE OF THE GRAND BANKS TURBIDITY CURRENT

PH. H. KUENEN

ABSTRACT. The observed velocity of what Heezen and Ewing have shown to be the Grand Banks turbidity current is used as a basis for estimating the size and sediment content. The volume of the original slides and the extent of the deposit are also evaluated. It is shown that a consistent picture of the whole process can be built up to fit the available data. The scale, though unexpectedly large, is by no means impossible as might at first sight be supposed. This supports the explanation of the cable breaks proposed by Heezen and Ewing. It also carries us a step further in understanding the emplacement of deep-sea sands and fossil graded beds. Needless to say the figures arrived at are merely suggested as a first approximation. It is argued that a slide of recent marine deposits may change to a turbulent flow without inmixing of water. They contain an ample supply of water to allow of turbulent flow once the thixotropic strength has been destroyed.

Whatever the result of future determination of volume for the deposit, it would be unreasonable to suppose this single case represents the upper limit in size ever attained in nature. A multiple of the area and thickness must be possible. If coring or examination of fossil graded beds should lead to volumes of dozens or even hundreds of cubic kilometers, this size can no longer be used as an argument against explaining the emplacement by turbidity flow.

IN the preceding enlightening article Heezen and Ewing show convincingly that the progressive sequence of breaks in cables accompanying the Grand Banks earthquake of 1929 cannot be explained by the direct action of the earth shocks or by undermining through slides. Neither do tsunamic waves account satisfactorily for the observed time lag between the first and last ruptures. The explanation they propose is that a gigantic turbidity current, resulting from slumps set off by the shock, caused the later cable breaks.

Although Bailey had suggested some time ago (1938), in connection with the erosion of canyons, that submarine slides could readily merge into submarine mud-rivers and Kuenen and Migliorini (1950) had invoked the same process to explain graded bedding and the emplacement of deep-sea sands (Kuenen, 1947 and 1950), while Kuenen (1950, p. 239) had attributed some other cable breaks tentatively to turibidity currents, no one had yet realized that here nature had carried out a vast experiment along these lines, accurately timed and located by the cable companies. Neither had anyone conceived of turbidity

currents, let alone experiments, on such an enormous scale.

The marshaled evidence appears to allow of no other interpretation and the picture offered is logical. Nevertheless many will feel doubt whether velocities of 100 km per hour and travel over hundreds of miles on an almost level bottom are within reasonable bounds of speculation. In the following the writer will attempt to show that this intuitive feeling is unfounded. When the size of the current and the amount of sediment involved are computed from the velocity, figures are obtained which are quite reasonable. The data will be seen to fit into an entirely consistent picture of what happened and this should strengthen confidence in the proposed explanation.

So little is yet known of turbidity currents in nature that no one can say offhand whether the thickness of the inferred current was to be measured in meters or hundreds of meters, whether the amount of sediment involved was a small fraction or a high multiple of a cubic kilometer, or whether the deposited bed should extend any distance beyond the farthest break or, finally, whether the deposit should be a few centimeters or dozens of meters thick. The writer hopes this will excuse a crude attempt to arrive at a first approximation and that it will be realized from the outset that the present results are not claimed to be more than tentative.

Before going any further it should first be examined whether a pure submarine slide could account for the observed phenomena not by undermining, but by impact. Two arguments appear to exclude this possibility. Firstly the slope at the lower end was so slight (less than 1/30 of a degree) that a slide would have stopped long before reaching the furthest breaking points. Secondly a slide cannot result in a graded deposit. Although deep-sea coring has revealed a great number of graded deep-sea sands, only a few small beds of unsorted coarse sediment have been found which are evidently due to local slides on rather steep slopes. If slides could attain the size indicated by the delayed cable breaks, deep-sea sampling should already have encountered many non-graded beds of uncorable thickness. Hence one may conclude that if the delayed breaks are due to sediment transport, this must have taken place in the shape of a turbidity current.

However, it might be conjectured that the breaks do not correspond to the velocity of the current, but occurred later

by gradual abrasion ("sand blasting") in the current. The active part of the current passed any point in a matter of minutes and the breaking must have occurred soon or not at all. The timing therefore cannot be changed significantly. Moreover, it would only mean having to assume even greater velocities. It appears safe to assume that the breaks occurred almost immediately after the current reached them.

On the basis of experiments and data on turbidity currents in reservoirs the present writer has attempted in earlier papers to deduce the relation between velocity on the one hand and size, density, and slope on the other. It was found, as Daly predicted, that the formula used for calculating the velocity of rivers $V = C\sqrt{m \times s \times d}$ ($V$ = velocity, $C$ = a constant, $m$ = hydraulic mean depth, $s$ = slope, $d$ = effective density) holds also for turbidity currents. These results will now be applied to the Grand Banks turbidity current.

The constant C is related to the internal and external friction and for turbidity currents the value must be lower than for rivers. In the centimeter-gram-second system C for large rivers is 700-800. For our case a value larger than 600 is out of the question.

In discussing his experiments the writer concluded (Kuenen, 1951) that C must be 125, but reconsideration has since led him to assume that the density in the experimental flows was smaller than supposed and hence C larger, probably about 200. The figure deduced for the turbidity currents in reservoirs was 400.

The highest density at which turbulent flow is possible is of the order of 2. But for an arbitrary mixture of grain sizes and as an average for the entire thickness of the flow it must necessarily be less.

Moreover, recent marine sediments contain from 60 to 80 per cent water by volume. A density of the current higher than that of the original sediment cannot be postulated. Hence it will be assumed that 60 per cent water by volume is the minimum, and this means a maximum density of 1.6 (effective density, $d = 0.6$).

As the currents appear to have been very broad the hydraulic mean depth is half the thickness of the current. In table 1 the thickness is calculated for various points along the bottom and for $C = 400$ and $D = 0.6$.

TABLE 1

| | Depth range in fathoms | Distance in miles | Slope | Velocity at end knots | Velocity at end meters/sec | in meters Hydr. m. d. | in meters Thickness |
|---|---|---|---|---|---|---|---|
| Cable H* ..... | ..... | ... | 1:170 | 55 | 28.3 | 137 | 270 |
| Cable H I ..... | 2160-2640 | 105 | 1:220 | 45 | 23.1 | 121 | 240 |
| Cable I J ..... | 2640-2760 | 130 | 1:1080 | 16 | 8.2 | 77 | 150 |
| Cable J K ..... | 2760-2773 | 20 | 1:1080 | 14 | 7.2 | 81 | 160 |
| Cable K L ..... | 2773-2800 | 40 | 1:1500 | 12 | 6.2 | 61 | 120 |
| Cable H L ..... | 2160-2800 | 295 | 1:440 | 32 | 16.4 | 123 | 240 |

C 400 d 0.6

* For lettering of cables see the preceding article.

In table 2 various values for C and d are tested for the entire run from breaking points H-L.

TABLE 2

| | C | d | slope | m/sec | Hydr. m. d. | Thickness |
|---|---|---|---|---|---|---|
| On basis of experiments ..... | 200 | 0.3 | 1:440 | 16.4 | 973 | 1940 |
| Maximum possible ..... | 600 | 0.8 | 1:440 | 16.4 | 41 | 80 |
| Most probable ..... | 400 | 0.6 | 1:440 | 16.4 | 123 | 240 |

From table 2 it follows that the values for C and d deduced from the experiments are much too low because they lead to an impossible size for the current. The minimum thickness possible is 80 meters. But if a figure of this magnitude has to be admitted it appears more likely C and d were less extreme and the thickness still greater.

Two significant points can be made here. If we are forced to assume a high average density the vertical component of the turbulence must be high to hold the sediment in suspension. This fits the enormous velocity established because turbulent velocities are related to velocity of flow. From the necessarily high value of C it follows that the surface of the flow experienced relatively light friction with the covering stagnant water. This, in turn, is a *conditio sine qua non* for maintaining the high density.

This result is confirmed by the figures in table 1. These show that the gradual decrease in velocity between Cables H to L must be attributed mainly to the diminishing grade. Only a moderate additional loss either in density or in thickness must have taken place. The table is based on the assumption of constant density. However, thinning due to lateral spreading is likely to have occurred. Some loss of sediment by deposition especially towards the far end is also probable. Obviously dilution by mixing cannot have been intensive, otherwise the current could not have maintained the observed velocity.

Doubt has been expressed whether the writer's former attempts to extrapolate from his experimental results to the dimension of turbidity currents in nature was permissible. It was suggested the higher velocities might result in increased friction and a much lower value for C. It now appears as if the opposite may be true and that the friction and dilution in the small scale experiments are *relatively* higher than with larger dimensions and swifter currents. For those who admit that graded beds of vast extent in fossil basins and on the ocean floor have been deposited from turbidity currents the above conclusion appears to be warranted. For only if friction and inmixing are limited can the phenomenon of turbidity flow attain the gigantic horizontal proportions which they are forced to assume.

Heezen and Ewing deduce the velocity of 55 knots at the first breaking point from the time-distance curve of their

figure 3. An attempt can be made to estimate where the current started and what its velocity was before reaching Cable H. The writer is inclined to assume that the original slides produced by the earth shock attained a high velocity and changed to turbidity currents almost immediately and that this took place on local steeper parts of the slope, somewhere around 1000 fathoms depth.

A slide without friction on a slope of 1 in 10 will attain a velocity of 30 m/sec in half a minute. The turbidity current is launched in a few minutes, or not at all, it may be assumed. We are further led to suppose that the sediment lying on the sea floor already contains all the water necessary for turbulent flow. The thixotropy of the clay and the loose packing of the sand account for the high porosity combined with a certain, though small, strength. The earthquake causes a large mass to start sliding and this movement then breaks down the internal strength. Thus the entire mass suddenly changes to a liquid. At this moment a turbulent turbidity current is launched.

This supposition was tested on some deep-sea samples of blue mud and tidal flat deposits. It was found that fine deep-sea lutite with 77 volume per cent water (density 1.33) if violently shaken in a bottle is soon mobilized. It runs like a watery liquid, but is pastelike when at rest due to strong thixotropy. The same is true of firm sandy muds from a tidal flat containing 58 volume per cent water and showing a density of 1.62. In the experiments it was also noted that a current with slight internal friction results when a viscous liquid starts to flow turbulently. Addition of an equal amount of sand to the lutite of the above deep-sea samples does not change the physical properties, except that the density is raised to 1.6. This mixture is the supposed composition of the Grand Banks slides.

Realization of the circumstance that the sediment requires no inmixing of water but only a physical treatment, to produce a liquid state clarifies an aspect of turbidity flow which had much puzzled the writer, namely how a slide could change to a turbulent flow.

The above reasoning concerning the launching leads one to suppose that the volume and thickness of the turbidity current increased to their maximum between 1000 and 1900 fathoms.

The velocity then gradually sank to 55 knots as it reached a slighter slope and passed the first point, gradually slackening to 12 knots at Cable L.

TABLE 3

| Depth range | Slope | C | d | Hydr. m. depth | Deduced Velocity— Knots | Meters/sec |
|---|---|---|---|---|---|---|
| 1000-2000 fath. | 1:50 | 400 | 1.6 | 150 | 10.5 | 54 |
| 1000-2000 fath. | 1:50 | 400 | 1.6 | 100 | 8.5 | 44 |
| 1000-2000 fath. | 1:50 | 400 | 1.6 | 85 | 7.8 | 40 |
| 1000-2000 fath. | 1:50 | 400 | 1.6 | 50 | 5.8 | 32 |

The velocity for various hydraulic mean depths, between 1000 and 2000 fathoms depth is shown in table 3. It is considered that the deduced decrease of thickness from Cable H to Cable L cannot have started much earlier. The preferred estimate for the average of m from the origin to Cable H is 85 meters (thickness 170 m). This would mean a thickness of 60 m at the origin at zero time, and 270 m when passing Cable H. The average velocity works out 40 m/sec or 78 knots.

Estimates as to the amount of sediment involved work out as follows: At its origin the flow was 60 meters thick and may be assumed to have had a length of at least 20 times as much, 1200 meters. For each meter of breadth the amount of sediment of a density of 1.6 was $72 \times 10^3$ m$^3$. The same amount was contained in the original slide imagined, which may have been 50 meters thick and 1500 meters long. Detailed knowledge of the bottom topography must be awaited before a more precise picture can be drawn of this initial phase of the current. Probably there were several slides, which may have merged into one or more turbidity currents.

By the time the turbidity current reached Cable H it must have been some 20 x 270 m long and would have contained $1400 \times 10^3$ m$^3$ of sediment per meter of breadth. This addition of $1328 \times 10^3$ m$^3$ came from bottom erosion over a distance of 122 kilometers and represents a layer 11 meters thick. The average length of the main body of the flow over this first part of the run was 3½ km and the current took 15 minutes to pass any fixed point. The erosion took place at 12 millimeters per second.

The amount of sediment required to give a velocity of 55 knots at Cable H is very large. If it had all come from a nearby slide, this slide would have to be 200 meters thick and 7 kilometers long. More probable is the supposed modest slide (or slides) and subsequent erosion on the steep slope above the first of the delayed cable breaks.

Naturally it is not supposed that the turbidity current showed an abrupt end. Behind the part considered above a tail of gradually diminishing density and velocity must have continued to flow for a long time. This tail may have deposited sediment in the wake of the eroding frontal portions.

The deduced thinning of the flow as it passed down the more gradual slope further along indicates that it was becoming longer and broader, possibly also less dense by the loss of coarse sediment. Erosion had probably ceased to exceed later deposition by the tail of the current by the time Cable J was reached, because this and the following cables were deeply buried.

A difficult point to evaluate is the influence on velocity due to thinning of the current. According to our estimates the center of gravity should have sunk with relation to the bottom by the following amounts: Cable H-I = 10 fathoms, I-J = 25 fathoms, J-L = 10 fathoms. The influence is negligible above Cable I, slight from I-J, but 25 per cent from J-L. In other words "internal slope" is adding to the effect of the bottom slope and the deduced thickness is too large. The best value lies around 100 meters.

The breadth of the current at Cable L was roughly 350 km. However, it is not probable that the front formed a smooth curve. On the slope the current will have shown concentration in density and in thickness along depressions. These fastest lobes will have caused the cable breaks. The intervening parts will have been thinner and less dense. Although these irregularities will have been gradually obliterated on the level ocean bed it is reasonable to suppose that this had not yet been fully attained at Cable L. Hence the total volume of wet sediment was less than is obtained by multiplying the lengthwise section by 350 km, let it be assumed one half or one fifth. This works out at 250 or 100 $km^3$.

A final point calling for attention is the distance to which the current may be expected to have spread. Assuming, to

start with, that the ocean floor is horizontal beyond Cable L, the centre of gravity at Cable L is 50 m above the floor at the limit to which the flow reached.

Fine sand can be transported by a turbidity current with a velocity of the order of 50 cm/sec. This velocity would have been attained some 120 miles beyond Cable L. The graded bed deposited by the current will gradually merge with the normal pelagic deep-sea sediment. It might still show up at 200 miles from Cable L over an area of perhaps 100,000 square miles. The average thickness would be 40 to 100 cm. Needless to say these figures are merely a guess. Their only value is to indicate that in spite of the huge velocity and volume of the current at the bottom of the continental rise, it is highly unlikely that the deposit could be recognized as far away as, say, Bermuda or along the eastern edge of the basin. A graded bed at the generally somewhat disturbed upper end of a core might not show up if less than several centimeters thick. Otherwise it might be claimed that coring should already have revealed its existence.

As the ocean floor appears to slope slightly to the south with some topographic irregularity one may expect the deposit to reach out in this direction in irregular lobes to even greater distances than the above estimate. It is also obvious that if the deposit can be recognized and its volume established by bottom sampling, a much better understanding of the turbidity current mechanism will be attained. If the volume is found to be many times our estimate, the original slides and the erosion by the turbidity current must both have been much larger, the constant C in the formula smaller than suggested. If it is much smaller the most probable explanation would be that there were a few separate currents of limited breadth.

We are now in a position to review the estimated values and inquire whether there is reason for considering any of them excessive.

*Velocity and size.*—The average velocity before reaching Cable H is estimated at 78 knots. The maximum must have been even greater, say 85 knots or 44 meters per second. The discharge per meter of front at Cable H was 7500 $m^3$ per second, the velocity 28 m/sec, the thickness 270 meters, the length of the main body 5½ kilometers. These values may appear altogether fantastic to many. But it should be borne in

mind that the available amount of water in the ocean is practically unlimited. Geologists and hydrologists are accustomed to think in terms of river dimensions. But to the oceanographer, who measures even surface currents with a discharge of 90 x $10^6$ $m^3$ sec (1000 times that of the Amazon River), it may well appear reasonable to postulate a bottom flow with a discharge of 7 x $10^6$ $m^3$/sec per kilometer of front. Likewise the velocity is not so excessive as may appear at first sight. Even a bore can travel up a river on a level surface at more than 13 knots, and velocities of 30 knots are claimed for the water in log chutes. The largest rivers carry but a trickle of water on an infinitesimal grade as compared to the deduced turbidity current. The grade above Cable L was 600 times that of the lower Mississippi River.

*Magnitude of initial slide.*—At the mouth of the Magdalene River deepening by a slide in 1935 amounted to 45 meters (Shepard, 1948, p. 196). In Sagami Bay at the time of the great Tokyo earthquake deepening reached no less than 180 meters. Our estimate of an initial slide of 50 meters thick is therefore quite reasoanble.

*Thickness of the flow.*—A number of submarine canyons show levees at their lower ends (Menard and Ludwick, 1951). These have evidently been built up by turbidity currents, which must have been able to lift sand to the level of the levee crests. Off California the bed of one canyon is at least 150 meters below the levee crests. In the Mediterranean values of more than 100 meters are indicated on a chart compiled by Bourcart. A thickness of 270 meters for the Grand Banks turbidity current is of the same order of magnitude (although the breadth was much greater).

*Thickness and extent of the deposited bed.*—Migliorini has found graded graywackes of more than 10 meters thick and extending for distances of at least many kilometers. The present estimate of an average thickness in the order of 1 meter is comparable. The failure to find the missing part of the last cable also tends to show that on a wide expanse of the ocean bed the deposit was more than a thin film. Ericson, Ewing and Heezen (1952) have discovered graded deep-sea sands which have travelled 1000 miles from the origin, almost twice the estimate for the present case.

In conclusion it appears that no inadmissibly large values for volume, thickness, or velocity either of the initial slide, the current itself, or the deposit need be assumed to build up a consistent picture of the postulated turbidity current. Hence these results tend to confirm the explanation Heezen and Ewing have offered for the delayed cable ruptures.

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Geologisch Instituut
Groningen, Netherlands

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# VENTRAL CEPHALIC SUTURES IN CAMBRIAN TRILOBITES

FRANCO RASETTI

ABSTRACT. The ventral portions of the cephalic sutures in Cambrian trilobites have seldom been studied and described. This paper attempts to survey the available information, on the basis of both published literature and new observations. The known types of cephalic sutures are defined and their occurrence in trilobites of the different epochs of the Cambrian period is reported. The evolutionary and taxonomic significance of the sutures is briefly discussed.

## INTRODUCTION

MOST of what we know about the cephalic sutures of trilobites had already been discovered by Barrande when, exactly one hundred years ago, he published the first volume of the monumental work *Système Silurien du centre de la Bohème*. Beecher's classification focused attention on the sutures, and the course of these lines, both on the dorsal and ventral sides of the carapace, is generally well known in the post-Cambrian forms. However, in the Cambrian genera and families only the dorsal portions of the cephalic sutures have usually received attention and are discussed in the descriptions of new species, the reason being the difficulty of obtaining specimens where the cephalic doublure is sufficiently well preserved and exposed to reveal the course of the sutures on the ventral side. Hence even recent studies of the sutures of trilobites (Richter, 1932; Stubblefield, 1936) had to rely largely on Barrande's observations for the Cambrian forms.

Preparation of a part of the forthcoming *Treatise on Invertebrate Paleontology* prompted the writer to undertake a survey of the cephalic sutures of Cambrian trilobites, based on both published information and firsthand study of material preserved in the United States National Museum and the writer's own collections. Since the course of the dorsal portions of the sutures is generally well known, the attention was focused on a study of the ventral sutures. Although the results of this investigation have been somewhat disappointing, owing to the exceedingly meager material, they are believed worth publishing, if only to emphasize the need for further research.

## TERMINOLOGY

The terminology here used was introduced by Barrande (1852) and employed with few modifications by Warburg (1925) and Stubblefield (1936). The "facial suture" is understood in Barrande's more comprehensive meaning of the *grande suture*, i. e., it includes the frontal portion that was set off by some authors as a "rostral suture." This distinction, at least in the Cambrian forms (and, in the writer's opinion, also in the later trilobites) is arbitrary and does not fulfill any descriptive or interpretative purpose. The "connective sutures" are a pair of sutures separating the doublure of the free cheeks from the median portion of the doublure (rostrum). The latter, in turn, may be separated from the hypostoma by a "hypostomal suture" or fused with this organ. In other trilobites the paired connective sutures are replaced by a "median suture"; in this case, the hypostomal suture separates the hypostoma from the free cheeks. Median or connective sutures may be lacking, the doublures of the free cheeks being fused into a single piece.

Whenever the meaning of such terms as "wide," "long," etc., might be ambiguous, they are qualified with the abbreviations "tr." (transverse) or "sag." (sagittal) to indicate measurements taken in the transverse or longitudinal directions, respectively.

## THE SUTURES IN CAMBRIAN GENERA AND FAMILIES

For the reasons already mentioned, the following discussion is essentially confined to the ventral sutures. This section reviews the published or new information concerning Cambrian genera and families. The next section attempts to discuss the evolution of the sutures and their taxonomic significance.

*Olenellidae.*—Recent authors (Stubblefield, 1936; Raw, 1937; Störmer, 1942) agree that there is a marginal or submarginal suture separating the dorsal shield from a ventral plate (rostrum) to which the hypostoma is attached. The hypostomal suture is in a state of symphysis in some of the genera. The difficult problem of homologizing the parts of the olenellid cephalon with those of opisthoparian trilobites has been exhaustively discussed in the cited literature.

*Agnostidae.*—As far as anybody has been able to ascertain, the agnostid cephalon lacks sutures and consists of a single

piece. Authors, beginning with Barrande (1852) searched in vain for agnostid hypostomas and suspected that these trilobites lacked that organ, at least in calcified form capable of fossilization. This suspicion is confirmed by new observations made by the writer on silicified, enrolled specimens of *Trinodus elspethi* (Raymond) from the Middle Ordovician limestones of Virginia. Tightly enrolled individuals were almost certainly buried alive, since muscle contraction must have been necessary to hold the shields pressed together; at least, it is virtually certain that such specimens do not represent moults. Hence, if the animal had a calcified hypostoma, it should now be present inside the tightly closed box formed by the enrolled shield. Examination of several perfectly silicified individuals, immersed in and filled with xylene, whose refractive index is close to that of quartz and allows a clear view of the space enclosed by the transparent carapace, failed to reveal any trace of the hypostoma, either in place or loose within the enrolled shield.

*Eodiscidae.*—No cephalic sutures are known in this family (Rasetti, 1948*a*, 1952*a*). No eodiscid hypostoma has been described; however, the close relationship with *Pagetia* makes it very likely that the Eodiscidae possessed this organ.

*Pagetiidae.*—These trilobites possess small free cheeks, widely separated from each other dorsally and presumably also ventrally, although no direct observation is available on the latter point, as the doublure of the free cheeks could not be studied in any species. The facial sutures on either side are thus apparently unconnected. Öpik (1952) recently described specimens of *Pagetia bootes* with the hypostoma in place. The manner of attachment of this organ has not been ascertained.

*Paradoxididae.*—The doublures of the free cheeks are separated by a wide (tr.) rostrum in all species of *Paradoxides* where these parts of the shield are known. The hypostoma seems to be fused with the rostrum in certain species, e. g., *Paradoxides bohemicus* (Barrande, 1852), a deep furrow marking the position of the fused hypostomal suture. In other species of the same genus (Westergård, 1937) the hypostomal suture seems to be functional.

In *Xystridura saint-smithi* (Chapman), according to Whitehouse (1939), the doublures of the free cheeks are also separated by a wide (tr.) rostrum, and "the hypostome is not permanently attached to the rostrum."

*Redlichiidae.*—Whitehouse (1939), Kobayashi (1944) and Kobayashi and Kato (1951) have shown that *Redlichia* has a wide (tr.) rostrum to which the hypostoma is probably fused. Whitehouse (1939, p. 192, plate 20, fig. 4) states that in *Redlichia idonea* Whitehouse "the hypostome . . . is either fused with the rostrum or tightly attached across the suture." Kobayashi (1944, plate 8, figs. 8, 9) figures a specimen of *Redlichia mansuyi* Resser and Endo where the hypostoma is attached to the rostrum although all the other cephalic parts are separated from each other. There is a depressed (in ventral view) area of the rostrum between the furrow corresponding to the anterior marginal furrow on the cranidium and the anterior margin of the hypostoma. In their paper on the morphology and ontogeny of *Redlichia chinensis* Walcott, the authors (Kobayashi and Kato, 1951) state that "in the present collection no specimen shows the mode of the hypostoma attachment clearly."

*Oryctocephalidae.*—Remarks on the ventral parts of the cephalon of *Oryctocephalus* were published by Saito (1934). Both in *Oryctocephalus orientalis* Saito and *Oryctocephalus* cf. *reynoldsi* Reed, he figured specimens (plate 25, fig. 18; plate 27, fig. 18) showing the free cheeks and hypostoma attached to each other without the remaining cephalic parts. Saito states

PLATE 1

Fig. 1. *Oryctocephalus walcotti* Resser. Ventral view of free cheeks and attached rostrum and hypostoma, flattened in shale, X4. Specimen no. 96503, U. S. N. M. Middle Cambrian (Spence shale); locality U. S. N. M. 55c, 5 mi. SW of Liberty, Idaho.

Fig. 2. *Fieldaspis furcata* Rasetti. Hypostoma fused with the rostrum, X3. Middle Cambrian (Mt. Whyte formation); Mt. Field, British Columbia.

Fig. 3. *Olenoides curticei* Walcott. Ventral view of cephalon, X2. Restored from specimens preserved in siliceous nodules of the Conasauga formation (upper Middle Cambrian); locality U. S. N. M. 90x, near Center, Alabama.

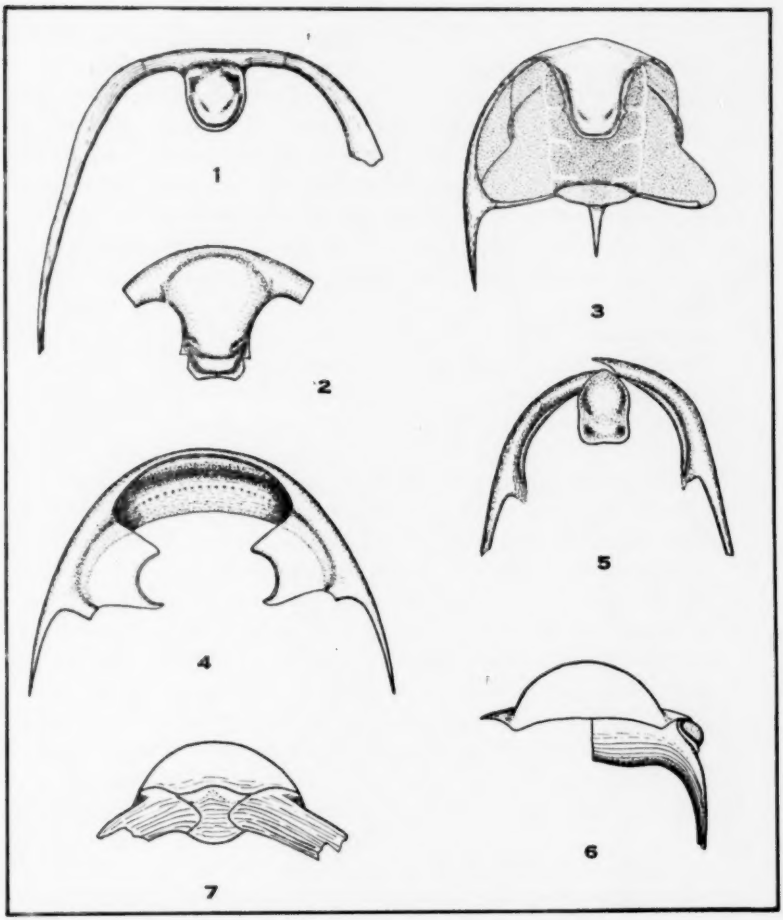
Fig. 4. *Levisella oweni* (Billings). Free cheeks, X3. Restored from specimens in the National Museum of Canada and the writer's collections. Upper Cambrian (Trempealeauian); boulders in Lévis conglomerate, Lévis, Quebec.

Fig. 5. *Bathynotus holopygus* (Hall). Ventral view of free cheeks and attached hypostoma, flattened in shale, X1. Specimen no. 15409, U. S. N. M. Lower Cambrian (Parker shale); Parker's quarry, Georgia, Vermont.

Fig. 6. *Acheilus latus* Rasetti. Anterior view of cephalon, X8. Specimen no. 1226, Laval University. Upper Cambrian (Trempealeauian); boulder in Lévis conglomerate, Lévis, Quebec.

Fig. 7. *Catillicephala impressa* (Rasetti). Antero-ventral view of cephalon, X6, showing the hourglass-shaped rostrum. Upper Cambrian (Dresbachian); boulder in conglomerate, Grosses Roches, Quebec.

PLATE 1



(p. 230) that in *Oryctocephalus orientalis* the free cheeks are "connected ventrally by doublure of anterior border"; and that in *Oryctocephalus* cf. *reynoldsi* (p. 232) the hypostoma is "apparently fused with the doublure of the anterior border." These statements would suggest that the free cheeks, rostrum and hypostoma are fused into a single piece.

The writer examined all the material of *Oryctocephalus* available in the U. S. National Museum. Unfortunately, specimens showing the ventral side of the cephalon are few and not too well preserved. Observations on shields of *O. burgessensis* Resser, *O. reynoldsi* Reed and *O. walcotti* Resser definitely indicate that the hypostoma is fused with the rostrum. This is also shown by a separated hypostoma of *O. primus* Walcott. It is more difficult to ascertain whether there are connective sutures. The specimens figured by Saito, and also a shield of *O. walcotti* figured by Resser (1939, plate 2, fig. 15), show that the free cheeks and rostrum are sometimes preserved in their normal relative positions, although displaced with respect to the cranidium, suggesting that the connective sutures are in a state of complete or partial symphysis. However, the facts may also be interpreted as indicating that the connective sutures were not used in ecdysis like the facial suture. In the above-mentioned specimen of *O. walcotti*, carefully studied and photographed, the connective sutures seem visible, although the possibility that a pair of cracks were produced in the flattening of the shield in the positions of the expected connective sutures is not entirely excluded. A drawing of the free cheeks and hypostoma of this specimen is shown in plate 1, figure 1. The rostrum, if correctly interpreted, is unusually wide (tr.) as it is also observed in the above-mentioned hypostoma of *O. primus*.

*Corynexochidae, Dolichometopidae, Dorypygidae, Zacanthoididae.*—These families are treated as one unit, the dividing lines being to a large extent arbitrary.

The hypostoma and rostrum in place have been observed by the writer in specimens of *Bathyuriscus, Kootenia, Olenoides, Ogygopsis* and other genera. In all instances there is conclusive evidence for the fusion of the hypostoma with the rostrum; the latter separates the doublures of the free cheeks. Fusion of the hypostoma and rostrum is complete in the Dorypygidae, where usually not even a furrow remains to indicate the boundary between the two areas. Good observations were made on shields of *Kootenia dawsoni* (Walcott) from the

*Ogygopsis* shale, and *Olenoides curticei* Walcott from siliceous nodules of the Conasauga formation. Several individuals of *O. curticei* preserve the hypostoma in place and show its anterior outline exactly fitting that of the cranidium. A reconstruction of the ventral appearance of the cephalon of this species is presented in plate 1, figure 3.

In numerous other representatives of the Corynexochidae, Dolichometopidae, Dorypygidae and Zacanthoididae, although the hypostoma was not observed in place, the structure of this organ leaves no doubt as to its manner of attachment. The portion of the shield usually described as the hypostoma includes the fused rostrum, sometimes separated from the main body of the hypostoma by a shallow transverse furrow. The laterally extended "anterior wings" in these families represent the rostrum, in most of the genera sharply truncated by connective sutures perpendicular to the cephalic margin. The typical "corynexochid" hypostoma of *Fieldaspis furcata* Rasetti is shown in plate 1, figure 2. Almost identical or similar hypostomas were observed in species of *Anoria, Athabaskia, Bathyuriscidella, Bathyuriscus, Bonnia, Corynexochides, Corynexochus, Dolichometopsis, Glossopleura, Ptarmigania, Ptarmiganoides, Zacanthoides,* and other genera. Fusion of the hypostoma and rostrum is the rule in these families. A possible exception is *Amphoton parallela* (Endo and Resser, 1937, plate 38, fig. 12); the hypostoma tentatively assigned to the species lacks the strong "anterior wings" and does not suggest direct attachment to the free cheeks. The fact that the hypostomas of other species of the genus (e. g., *Amphoton alia,* Endo and Resser, 1937, plate 38, fig. 16) show the usual corynexochid structure casts doubt on the assignment of the hypostoma in the former case. In *Ogygopsis* the hypostoma and rostrum are sometimes found attached, at other times separated, indicating a line of weakness if not a really functional suture.

*Bathynotus.*—In the Lower Cambrian trilobite *Bathynotus holopygus* (Hall) the doublures of the free cheeks are joined to the hypostoma through a pair of sutures directed obliquely forward and inward (plate 1, fig. 5). Although half a dozen specimens in shale exposed from the ventral side are extant, the writer was unable to ascertain whether or not the free cheeks meet for a short distance on the median line near the anterior margin. In any case it is certain that there is no rostrum, either separate or fused with the hypostoma, a feature

that seems to distinguish ***Bathynotus*** from any other known Lower Cambrian opisthoparian. If the free cheeks meet on the median line, the type of cephalic sutures is essentially the same as in the much later (Upper Cambrian and Ordovician) family Asaphidae. However, partly because the contact of the free cheeks in ***Bathynotus*** is uncertain, partly because we have reason to believe that the asaphid suture was independently developed, we shall assign ***Bathynotus*** a special position. The ventral sutures in the closely related Middle Cambrian ***Bathynotellus*** (Lermontova, 1940) are unknown.

*Conocoryphidae, Ellipsocephalidae, Ptychopariidae.*—For the knowledge of the ventral sutures in these groups we must depend on Barrande's observations. Among the hundreds of species of Lower and Middle Cambrian ptychopariids studied, the writer was unable to locate specimens showing the ventral parts of the cephalic carapace in place.

Barrande states that ***Conocoryphe sulzeri***, ***Ellipsocephalus hoffi***, ***Ptychoparia striata***, and ***Sao hirsuta*** possess a rostrum separated from the doublures of the free cheeks by connective sutures. He figured restorations of the ventral views of the cephala of ***Conocoryphe sulzeri*** and ***Ptychoparia striata*** (Barrande, 1852, plate 2b, figs. 24, 26). However, the relative positions of the various parts seem to have been inferred from observation of separate fragments rather than connected cephala, since the only actual individuals figured show only a portion of the hypostoma in place (plate 14, figs. 3, 10) and do not indicate the manner of attachment of this organ. That these trilobites and all their close relatives possess a rostrum seems certain, and also that the hypostoma is never fused with the rostrum. It is even possible that, at least in certain genera, the hypostoma was not directly attached to the rostrum, but separated from it by an uncalcified membrane. This possibility is suggested by the observation that in many ptychopariids there is a considerable distance between the front of the glabella and the anterior marginal furrow where the cephalic doublure apparently terminates. The hypostoma was certainly located under the anterior portion of the glabella, as shown by many crushed specimens where the outline of the hypostoma was impressed on the dorsal shield. There seems to remain an unexplained distance between the posterior margin of the doublure (rostrum) and the anterior margin of the hypostoma, a space that would be occupied by the above-mentioned uncalcified

portion of the ventral integument. Further study of better-preserved material is necessary to decide this point. A less likely assumption is that the hypostoma was attached to the doublure by a stalk, as in the olenellid genus *Paedeumias* (Resser and Howell, 1938, plate 9, figs. 6, 7).

*Upper Cambrian trilobites.*—Many of the proposed families of Upper Cambrian trilobites are probably heterogeneous assemblages not representing phylogenetic units. As there is no certainty that all members of such families present the same type of sutures, genera and species where the ventral sutures are known are individually discussed.

In *Dikelocephalus retrorsus*, *D. subplanus*, and other species of the genus described by Ulrich and Resser (1930) specimens are found showing the free cheeks fused through a wide (sag.) doublure, with the hypostoma in place attached to the posterior margin of the doublure. According to the same authors, in other species of the genus (e. g., *D. raaschi* Ulrich and Resser) almost indistinguishable from the preceding in other characters, there is a median suture. Examination of material of these three species in the U. S. National Museum collections confirms Ulrich and Resser's conclusions.

Fused free cheeks were observed and in part reported by the writer (Rasetti, 1944, 1945*a*) in several species of Trempealeauian age from the Levis conglomerate. These are *Hungaia magnifica* (Billings), *Lauzonella planifrons* (Billings), *Levisella brevifrons* Rasetti, *L. oweni* (Billings), *Loganellus macropleurus* Rasetti, and "*Platycolpus*" *capax* (Billings). As a typical example of this type of suture, the free cheeks of *Levisella oweni* are represented in plate 1, figure 4. The writer (Rasetti, 1952*b*) is also describing fused doublures of the free cheeks in *Entomaspis radiatus* Ulrich in Bridge.

A median suture was reported by the writer (Rasetti, 1945*b*) in *Acheilus latus* Rasetti (plate 1, fig. 6). New observations revealed this type of suture also in *Acheilus spinosus* Rasetti, *Leiocoryphe gemma* Clark, and *Stenopilus elongatus* Rasetti. However, careful study failed to show the median suture in complete cephala of *Leiocoryphe transversa* Rasetti and it is possible that the doublures of the free cheeks are fused in this species. Separated free cheeks of *Housia canadensis* (Walcott) from the Goodsir formation (Franconian) of Moose Creek, British Columbia (specimens U.S.N.M. No. 5584) indicate a wide (sag.) doublure terminated by a median suture. This is the

earliest occurrence of this type of suture known to the writer.

Presence of a rostrum was reported (Rasetti, 1946) only in *Catillicephala impressa* (= *Cephalocoelia*) from boulders of Dresbachian age. A ventral view of the cephalon of this species is shown in plate 1, figure 7.

This short list includes all the firsthand evidence that the writer was able to collect about the character of the ventral sutures in Upper Cambrian trilobites. However, the search of the relevant literature cannot be considered exhaustive.

At this point it should be mentioned that R. Richter (1932) and Stubblefield (1936) refer to unpublished observations by Resser of Lower Cambrian trilobites possessing a type of suture not listed above, consisting of facial sutures extending onto the doublure and not connected by a marginal (rostral) suture. All the genera of Lower Cambrian trilobites in the U. S. National Museum having been examined, the writer can state with certainty that such sutures do not exist among them, unless the above description applies to the sutures of the Pagetiidae, which, however, almost certainly were not the object of Resser's remarks. Resser's unpublished, erroneous observations led R. Richter to uphold the theory that the facial sutures form one unit with the connective sutures, and that the marginal (rostral) suture represents a later development. One of the results of the present study is the accumulation of extremely strong evidence against this view and in favor of Barrande's conception of the *grande suture* as a unit. Among the hundreds of genera of Cambrian opisthoparian trilobites examined by the writer in the course of his studies, very few (e. g., *Catillicephala*), and none of these Lower or Middle Cambrian, show the connective suture as the natural continuation of the dorsal portion of the facial suture. Usually the *grande suture* does not form any angle at the points where it is joined by the connective sutures (if any), so that examination of the cranidium does not enable one to establish whether or not there are connective sutures, or, if their presence is known, to determine their location. This statement applies regardless of the position of the frontal portion of the *grande suture*, whether marginal, dorsal-intramarginal, or ventral-intramarginal. Setting off a portion of this suture as a "rostral suture" appears both arbitrary and useless.

## EVOLUTION OF THE CEPHALIC SUTURES

Let us first review the types of ventral cephalic sutures

observed in Cambrian trilobites in the order of their appearance.

In the Early Cambrian we observe a number of different types of ventral cephalic sutures, which we designate by a characteristic genus or family. These are: (1) the olenellid type, which might be further subdivided depending on whether or not the hypostoma is fused with the rostrum; (2) the agnostid type, i. e., lack of all sutures and probably of a hypostoma; (3) the eodiscid type, similar to the preceding except for the probable presence of the hypostoma; (4) the pagetiid type; (5) the corynexochid type, i. e., doublures of the free cheeks separated by the rostrum which is fused with the hypostoma; (6) the ptychopariid type, differing from the preceding in the lack of fusion of the hypostoma with the rostrum; (7) the bathynotid type.

In the Medial Cambrian all these types of sutures are presumably still represented by the respective groups, with the exception of the Olenellidae. No new types of sutures are known to have arisen through this epoch.

In the Late Cambrian, the Eodiscidae and Pagetiidae with their respective types of sutures become extinct. The agnostids persist through this epoch and the succeeding Ordovician period. Trilobites with corynexochid sutures become exceedingly rare. The only case known to the writer where the shape of the hypostoma definitely indicates fusion with the rostrum is *Hemirhodon* (Rasetti, 1946, 1948*b*) of late Medial Cambrian and early Late Cambrian age. Most of the Upper Cambrian opisthoparian trilobites seem to possess ptychopariid sutures, although this statement would at present be difficult to substantiate with actual observations. However, it is made highly plausible both by the obvious ptychopariid affinities of a great number of Upper Cambrian genera, and by the fact that this type of suture is of usual occurrence in the post-Cambrian families (Calymenidae, Otarionidae, Dimeropygidae, Proetidae) of almost certain ptychopariid descent.

Two new types of sutures are first known with certainty among medial and late Upper Cambrian (Franconian and Trempealeauian) trilobites, although in view of the small number of genera where the ventral sutures are known, we cannot exclude that these types had appeared earlier. In either of these cases the rostrum has been eliminated, the free cheeks meeting on the median line where they may be separated by a median suture (8, asaphid type) or fused together (9, levisellid type). The

hypostoma remains separated from the doublure of the free cheeks by a functional hypostomal suture. The former type of suture is well known in the Ordovician Asaphidae, the latter in the early Ordovician genera *Nileus* and *Symphysurus*. Fusion of the doublures of the free cheeks also occurs in the "hypoparian" families and in the proparian family Phacopidae.

The affinities and systematic position of the Olenellidae, and in particular the important and difficult question whether they gave rise to opisthoparian forms, were exhaustively discussed by several authors (Stubblefield, 1936; Raw, 1937; Störmer, 1942), and the writer, having no new facts or viewpoints to present, leaves this group out of consideration. Furthermore, since he is satisfied that the eodiscids and the agnostids left no descendants, the following discussion is confined to the significance of the ventral cephalic sutures for the phylogeny and taxonomy of the opisthoparian trilobites (which include the post-Cambrian "hypoparian" families).

The overwhelming majority of Lower and Middle Cambrian opisthoparians possessed a rostrum (the only known exception is *Bathynotus*) either fused with the hypostoma (corynexochid type) or separated from it (ptychopariid type). There are no direct observations of the ventral sutures in Lower Cambrian ptychopariids, but by analogy with the Middle Cambrian forms we may safely assume that they had the same type of suture. We have yet no evidence to decide whether these types of sutures were independently inherited from Precambrian ancestors, or developed one from the other, and, if so, which was the primitive one. In certain families, presence or absence of a hypostomal suture does not seem to possess much significance (Olenellidae, *Paradoxides*). However, no example of fusion is known in the ptychopariids, where a functional hypostomal suture, or even an uncalcified membrane, seems to be a constant characteristic.

The asaphid and levisellid types appear much later and almost certainly derive from one or both of the previous types—more probably from the ptychopariid type, as both asaphid and levisellid sutures always include a functional hypostomal suture. This agrees with the fact that most of the above-mentioned Upper Cambrian genera showing levisellid or asaphid sutures appear on other grounds as descendants of the ptychopariid stock (which seems true of the overwhelming majority of the known Upper Cambrian genera). An alternative hypothesis is that *Bathynotus* represents the ancestral stock of trilobites

with asaphid sutures, since it already lacked a rostrum; but it is an unlikely assumption as we know no intermediate forms connecting *Bathynotus* with the Asaphidae. The derivation of the asaphid and levisellid types of sutures from the ptychopariid type is easy to imagine, the connective sutures approaching each other until they merge into a median suture, or becoming fused either before or after coalescing into a single suture.

Among the post-Cambrian opisthoparian trilobites, the ptychopariid type of suture is the most common, followed by the asaphid and levisellid types in order of frequency. Fusion of the hypostoma with the rostrum seems unknown. It should be noted that the ptychopariid type of suture also occurs in those families (Illaenidae, Scutellidae) which, from the general appearance, have been considered (Richter, 1932) as descendants of the corynexochid stock. The lack of fusion of the hypostoma with the rostrum may be an indication against this assumed ancestry, when coupled with the fact that corynexochid trilobites seem almost entirely to disappear with the end of the Middle Cambrian and we are unable to connect them with the above-mentioned families. Unfortunately, we are at present unable to trace the Cambrian ancestry of many other post-Cambrian families, e. g., the Lichadidae, Odontopleuridae, and the proparian Cheiruridae, Encrinuridae and Phacopidae.

To assess the value of the ventral cephalic sutures for the phylogeny and taxonomy of the Cambrian genera, it will be necessary to know the types of sutures in a much greater number of forms. It does not seem plausible to expect that a phylogenetic classification of the Cambrian trilobites will be based on this character alone, or, for that matter, on any other single feature. The more we know about Cambrian trilobites, the stronger the conviction that there are no clear-cut characters of the exoskeleton that can be used for major subdivisions. A rational classification can emerge only from observing gradual changes of species and genera in space and time and establishing lines of descent. What we already know indicates that the task will be made difficult by widespread parallelism and convergence. It is possible that knowledge of the ventral sutures may furnish the clue for separating superficially similar but distantly related phylogenetic series.

It is hoped that this brief survey of the little we know about the ventral sutures in Cambrian trilobites will stimulate much-needed new observations. The following points are especially

suggested to the attention of paleontologists: (1) observation of the ventral integument in Lower and Middle Cambrian ptychopariids, to establish the manner of attachment of the hypostoma to the rostrum; (2) study of the earliest appearance of the asaphid and levisellid types of sutures; (3) observation of the change in types of cephalic sutures in fairly well established phylogenetic series, to determine in what directions sutures have evolved, e. g., whether changes consisted in fusion of pre-existing sutures or appearance of new ones.

Both fossils in shale and limestone can be valuable in supplying this information. When collecting trilobites from shales where entire shields are not uncommon, attention should be paid to specimens exposed from the ventral side (unfortunately neglected by most collectors), which may preserve the hypostoma in place. Free cheeks separated from the cranidium but still attached to each other may furnish evidence for the levisellid type of suture. Cephala in limestone preserving the free cheeks should be carefully prepared to show the anterior doublure. Even isolated free cheeks, when the doublure is carefully exposed, may prove whether or not it extended to the midline, indicating absence or presence of the rostrum. Studies of this type will add far more to our knowledge of these organisms than the descriptions of endless new species, especially when based on insignificant differences from already known forms.

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The Johns Hopkins University
Baltimore, Maryland

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# STREAM COMPETENCE AND THE GRADED STREAM PROFILE

CHAUNCEY D. HOLMES

ABSTRACT. A review of factors governing the equilibrium profile of streams leads to the conclusion that competence at the channel-floor level is the most fundamental factor. Although this is implicit in several recent discussions of the problem, its full geologic significance seems not to have been adequately stated. The graded condition may evolve gradually and simultaneously throughout most of the stream course inasmuch as competence determines the gradient for each and all of the several segments finally integrated (the equilibrium profile). The graded condition is probably attained some time before it can be detected. Stream capacity as defined has analytical significance but its effect on the equilibrium profile is indirect.

## INTRODUCTION

RECOGNITION of an equilibrium stream profile leading down to a controlling baselevel was one of the fundamental generalizations on which the concept of the erosion cycle came to be based. The shift from predominant valley deepening to predominant widening marks a significant stage in valley history. It coincides with attainment of the equilibrium profile, and the coincidence has sometimes obscured the fundamental controls that give rise to these conditions. The factor most commonly mentioned in discussion of graded streams is that of stream load, with attention centered largely on stream capacity.

Among the recent contributions to the subject of stream-profile equilibrium are the excellent papers by Mackin (1948) and Woodford (1951). These authors discussed competence as well as capacity, and in this connection Woodford, (1951, p. 817), stated that "pebble size is the chief determinant of gradient." Mackin (1948, p. 505) implied a similar view. The present writer believes this thought deserves more than passing mention, and the purpose of this paper is to supplement these recent discussions and to emphasize stream competence as the primary controlling factor in the graded stream profile.

## APPROACH TO THE GRADED CONDITION

As Mackin (1948) stated, the graded profile is an adjustable transportation slope maintained through a long time by chan-

nel deposition or erosion as required in response to factors of bed load and discharge, modified by shape and character of channel and character of total load. In order to detect the existence or nonexistence of grade, it is necessary to know the erosional history of the stream as inferred from such evidence as strath terraces and character of alluvium. This raises the question of how early in its development the graded condition may be identified.

Consideration of the hypothetical short-term changes as grade is approached may be helpful in an attempt to reconstruct the transition from active downcutting to planation. First, some channel fill deposited during a falling river stage may not be removed during the next normal high stage, a condition that should occur with increasing frequency and extent along the channel until the entire amount is swept away only during occasional very high stages. Alluvium will then normally constitute the stream bed except for short periods of time during the greatest floods. Observation over a long period would be required to detect the arrival of this condition, and probably not until perceptible valley widening had gone on could the presence of an equilibrium gradient be identified with assurance. In other words, the graded condition is probably attained long before its existence can be demonstrated. Slow continued downcutting during the higher discharge stages is normal to the graded condition and is an essential adjustment to the progress of the erosion cycle in which the particular drainage system (or any segment of it) plays a unit part.

An important requirement in achieving and maintaining the graded condition is that the channel fill shall not contain material coarser than can be removed progressively during the rising phase. Otherwise, the fill becomes equivalent to a bedrock channel with gradient higher than is required for equilibrium. Immediate and perfect adjustment may not always be attained where the fill consists of a wide range of grade sizes, because during rising stage the channel floor will consist of a lag concentrate of progressively coarser sizes. But this only serves to emphasize the fact that competence to move the largest fragments in the channel, down to the required depth or level, is the ultimate test of whether the stream gradient can be kept in adjustment. It means also that the adjacent upstream segment should not deliver sediment of larger maximum size than the segment under consideration can con-

tinue to transport without undue delay. This implies that a reasonably close approach to the graded condition must be attained through a considerable distance upstream before any lower segment can maintain a graded profile. In other words, the graded condition in any stream segment requires that the several segments be so integrated that a change in the controlling factors in any one segment will cause a displacement of the equilibrium throughout all the others thus integrated, "tending to absorb the effect of the change" (Mackin, 1948, p. 471). It may be pertinent therefore to inquire whether the graded profile is initiated at the mouth of the stream and extended upvalley, or whether it evolves somewhat simultaneously throughout the entire length of the stream, except possibly the small headwater tributaries. Perhaps in any given stream segment the graded condition is attained for a time and then lost temporarily until the upstream segments also become graded. This may well be the case inasmuch as attainment of grade may long precede its recognition. In this consideration the upstream tributaries are, in proportion to their sizes and other pertinent characteristics, as important as the main drainage channel itself.

During the early period of adjusted gradient the loads carried by most streams may be assumed to show a wider range of clastic grade sizes than would be the case at any later time, because, assuming constant discharge, gradient can be reduced progressively under natural conditions only in response to a lower required value for competence, or in a reduction of total load. (This is discussed on a later page.) Bank caving during valley widening may be presumed to supply coarse as well as fine sediment. As caving occurs, fragments coarser than the limit of competence must await further reduction by weathering agents, and this may be taken to mean that a graded profile cannot be assured until the valley walls also have reached a condition of approximate grade so that the amount falling into the channel at any one time will not involve adjustments beyond the limits within which the stream must function in maintaining grade. Bank caving along the whole stream course is a load factor from the beginning, and the fact that it continues by lateral planation need not imply an added demand on transport. It was already a part of the regulating mechanism when the equilibrium gradient was attained.

The fact that streams are almost never fully loaded with

the finer grade sizes is now generally recognized. This explains why a graded stream can take up added load without the necessity of increasing its gradient. The stream is "fully loaded" only with respect to the coarsest part of its load, which is another way of stating that its gradient is adjusted to the competence required to move these coarsest fragments. Merely to say that the gradient is adjusted to the stream load is sufficient only when the significant factors are understood. It is necessary to state which part of the load is critically involved; and because for virtually all streams the coarsest fraction is the critical one, the real controlling factor becomes a matter of competence rather than of capacity as that term is now commonly used.

### VALLEY WIDENING

Lateral deflection of stream flow is present everywhere from the very beginning of any given stream history, and it seems safe to assume that the initial development of a sinuous course, as lateral planation begins, is due to current deflection caused by differential channel erosion or by accidental local channel obstruction at that particular time. Russell (1936) concluded that a distinction should be made between curves initiated under such conditions and the meanders typical of rivers (such as the Mississippi) flowing through a wide alluvial plain where differential channel erosion as the river rises may be relatively more effective. The beginning of lateral planation is followed directly by meander development, which in turn lengthens the river course. This involves necessarily a reduction in gradient and therefore reduction in competence inasmuch as grade was achieved with reference to the more direct course to the controlling baselevel. Actual aggradation to maintain gradient may not be necessary because of the trend toward smaller maximum grade size delivered to the stream segment under consideration as the erosion cycle progresses. A possible variation here may arise from relatively low available relief (Glock, 1932) in which regional topographic maturity, and therefore maximum supply of sediment, is delayed appreciably past the stage of initial strath development. In regions where the available relief has at least critical value, lengthening by meander development may have the effect chiefly of delaying or reducing the rate of slow downcutting that

would otherwise attend the decline in maximum grade size to be transported. Once a stream has reached its maximum length by fully developed meanders, the rate of further deepening should increase. This may well occur before the strath is fully formed.

### THE "OVERLOADED" STREAM

Discussion thus far has concerned chiefly those streams whose load does not include the maximum possible amount or proportion of the smaller grade sizes, and whose gradients are therefore governed by adequate quantities of the coarser sizes,—that is, material classed as gravel. Such conditions are apparently common in many streams, especially during the earlier phases of the graded condition, and they obviously depend on a continuing supply of these coarser sizes.

A somewhat different situation seems to exist where the stream load consists of a superabundance of the smaller grade sizes (sand and silt) and a near absence of gravel. Whipple (1942, p. 1200) stated in regard to the Missouri River between Rulo, Nebraska, and Sioux City, Iowa: "Since the Missouri River does not now find in its bed sediments as coarse as those which it is competent to carry at high discharge, it is concluded that bed scour of this stream is limited by capacity, and that formulas for competence will not define an equilibrium condition in this case." In this segment the river load was (and presumably is) predominantly sand and silt with only a small amount of fine gravel. In reality the situation would not be essentially different were the load almost entirely of fine gravel, but in sufficiently large quantity and of course with a correspondingly steeper gradient. Capacity in this sense would still appear as the limiting factor. However, sand and silt are far more common in these situations. In the more pronounced cases the stream channel becomes anastomosing at low-water stage and the stream is commonly said to be overloaded.

The writer has long believed that the term "overloaded stream" does not express the real situation and is likely to convey an erroneous impression to the uninitiated. Actually a stream cannot be overloaded because its load can never exceed the limits of the mechanical conditions at any given place and time. The situation is not comparable to that of an overloaded electrical circuit, or of an overloaded test beam, in which con-

tinuance of the load results finally in breakdown of the system. A stream may be fully loaded but never overloaded. The hydraulic energy may be applied in ways difficult to decipher exactly and completely, but so long as a particle of sediment continues in motion the energy of the stream is quite adequate for the effect produced and is not overtaxed. At the instant the particle stops, force and resistance have become equal at that particular place and time. The general concept of capacity as used in such cases merits some further thought.

### COMPETENCE IN RELATION TO VOLUME OF MOVING SEDIMENT

In turbulent flow, velocity and even direction of flow past any given point on the stream bed are subject to considerable variation and the fluctuations are too rapid to admit of close measurements. They are known to be very effective in causing saltation of sedimentary particles and in keeping the finer sediments in suspension. As Rubey (1938) has pointed out, the place where velocity is effective in transportation is where the sediment is being transported—that is, near the stream bed. The "bed velocity" may be much less than the average velocity but it is the significant factor in transportation. Each sand grain moved represents work done and so slackens the water velocity a slight amount. Sand grains in sufficient quantity comprising the bed load not only reduce bed velocity (and therefore competence) but at the same time they impede turbulence probably to the point of eliminating it altogether as it is initially dependent on velocity. Though transport of sand continues briskly in the upper zone of the bed load, the rate becomes much slower with increasing depth down to the level at which the velocity of water flowing past the sand grains is no longer competent to move them forward. Under these conditions, actual velocity near the bed probably cannot be measured accurately (Rubey, 1933). Whatever may be the surface velocity or the average velocity of such a stream, the amount of bed-load sand and silt necessary to reduce the velocity at the bottom below the limit of competence for the particle sizes at that depth, establishes the equilibrium gradient. This is essentially the manner in which gradient is determined by a gravel-sized load, and in any case the coarsest grade sizes are to be found on the channel floor. The stream leaves a given clastic particle at the place where stream competence

declines below the value required to keep the particle in motion. Volume of load is therefore effective as a controlling factor only as it affects the conditions under which competence sets the ultimate limit. Each graded stream has its own unique ratio among its several controlling factors, and the differences among graded streams are believed to be differences of degree rather than of kind.

Stream capacity—"the maximum load a stream can carry"—has always proved to be an elusive quantity, and it may remain forever impossible to ascertain, although the theory is wholly logical. Given a certain gradient and discharge and stable channel conditions, it should be possible to find the optimum ratio of all grade sizes, up to the limit of competence, that would represent the maximum possible load. But natural channels are not constant in cross section (channel efficiency) or in other characteristics. Even though conditions of transport were stable and uniform, mutual abrasion among the particles in transit constantly change any existing ratio, resulting generally in a deficiency of the finer sizes (fine silt and clay). Additional load in these sizes may be available, but clay in particular does not come into suspension readily from the channel floor. Also, higher velocity is necessary to set a particle in motion than is required to keep it moving.

Mackin (1948, p. 505) has stressed the fact that the rate of transport of the finer sizes greatly exceeds that of the coarser. Even though the relative percentages of the several grade sizes present in a stream at a given place and time can be measured, the actual ratios of the finer grade sizes transported in a given time interval will be proportionally higher because of their greater downstream velocity. This means that the "maximum load a stream can carry" does not indicate "the maximum volume or mass of sediment the stream can deliver in a unit of time." While it is true that the former can be correlated with stream gradient for purposes of engineering analysis, the latter is of primary geologic significance because it expresses more accurately the rate at which erosion is progressing and the rate at which clastic sediment is being delivered to the depositional basin.

The conclusions suggested by these thoughts are that stream capacity is a somewhat ambiguous term denoting a variable but unknown quantity, and that graded streams of identical

gradient and discharge may still vary widely in the "maximum loads" which they are observed to carry. But with due allowance for the existing conditions, their stream-bed competence values should be directly comparable.

## CONCLUSIONS

From the geologic standpoint of progressive changes through the erosion cycle, the two most significant factors appear to be: (1) the total amount and grade-size ratios of sediment transported in a given time interval, and (2) the equilibrium gradient on which transportation is accomplished. But considering that each sedimentary particle has its own unique rate of downstream movement governed by its size, shape, and specific gravity, the actual amount (volume) of sediment passing any point in a given time interval cannot be determined by merely measuring the amount of load in transit at that time and place. The varying rates of transport must be taken into consideration. In this respect, the "maximum load a stream can carry" seems even more difficult to determine, and attempts to understand gradient in terms of momentary load and capacity become correspondingly unfruitful.

The ultimate control which competence has on gradient is implicit in the discussions by most authors. At least for geologic purposes, clarity of thought on equilibrium gradients and related problems would seem therefore to be promoted by centering attention directly on the factors that influence "stream-bed competence."

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UNIVERSITY OF MISSOURI
COLUMBIA, MISSOURI

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# DATING OF PLUVIAL LAKES IN THE GREAT BASIN

IRA S. ALLISON

ABSTRACT. The uppermost pluvial-lake shorelines in south-central Oregon are considered pre-Wisconsin in age. On the basis of a pollen and pumice record, a low-level beach in Chewaucan-Abert basin, Oregon, is assigned to Mankato time and is correlated with the Stansbury phase of Lake Bonneville which is dated by the radiocarbon method. The correlations of the intermediate strandlines of Bonneville, Lahontan and other pluvial lakes require adjustment.

THE work of Flint and Deevey (1951) in interpreting the glacial and pollen records in the light of radiocarbon dating is an important contribution to our understanding of late Pleistocene events. One part of the record, however, not touched upon by these authors, i.e., the ages of the pluvial lakes in the Great Basin, deserves comment.

Influenced by Antevs and others, the writer (Allison, 1945) once correlated the highest pluvial shorelines in the Summer Lake basin, Oregon, with the Bonneville phase of Lake Bonneville, Utah, and a lower, well-developed shoreline with the Provo phase. These were thought to be of early and late Wisconsin ages, respectively, and equivalent to the Tahoe and Tioga glacial stages of the Sierra Nevada.

Detailed studies of these shorelines, aided by profiles obtained by leveling in 1947, cast doubt upon the age assignments. In the Summer Lake basin, for example, in addition to the outstanding 4500- and 4365-foot levels, there are strong shorelines (or swarms of them) at elevations of about 4400 and 4325 feet, and other weaker ones—too many to fit the earlier correlation. By 1950 the writer had decided that at least the uppermost levels of the former lakes were pre-Wisconsin and so reported at the Seattle meeting of the Cordilleran Section, Geological Society of America (Allison, 1950).

At the opposite or young end of the series, one of the factors tending to indicate a shallower depth during the last pluvial lake phase than previously envisioned is the pollen profile obtained by Hansen (1947*a*) at Upper Chewaucan Marsh near Paisley, Oregon. This pollen record, 2.4 meters in depth, shows an early zone indicating a cool moist climate, a middle zone signifying maximum warmth and dryness, and a late zone

denoting cooler and moister conditions—a record comparable in its completeness with many others in the Pacific Northwest that include all of "postglacial" time (Hansen, 1947*b*). Hansen remarked (1947*a*, p. 166) that the pluvial lakes must have shoaled rapidly to permit deposition of the peat over such a large part of postpluvial time. Instead, the pluvial lake was not as deep as formerly supposed; hence less shoaling was required.

Pumice of Mount Mazama (Crater Lake) origin is imbedded in the peat at a depth of 1.2 meters. The Mount Mazama pumice was dated in 1951 at 6,453 ± 250 years by the radiocarbon method from a sample of charred wood obtained in the upper Rogue River valley just west of Crater Lake National Park (Arnold and Libby, 1951). The climatic history furnished by the pollen itself and the position of the 6,450-year-old pumice, midway in the peat section, tend to show that the time recorded by the peat includes substantially all of postglacial or postpluvial time. Indeed, the lowest 0.4 meter of the peat may extend back into pluvial time, as it is silty and rests on pebbly gravel. If the bottom half of the deposit accumulated at the same rate as the top half, then the entire deposit should cover a span of nearly 13,000 years. As only the top 0.3 meter is fibrous sedge peat and 1.7 meters of the remainder is limnic, the time represented may really be longer than 13,000 years.

This peat record, dated by pumice in the middle, leads the writer to assign a late-Wisconsin (Mankato-Valders or Tioga) age to the broad beach overlooking the north end of Upper Chewaucan Marsh at an elevation of 4,325 feet, only 25 to 30 feet above the north end of the Marsh itself and only about 95 feet above modern Abert Lake in the bottom of the fault basin. For convenience this may be called the ZX Red Barn beach, from the ranch building of that name situated on it.

Two dates given by Libby (1951; 1952, pp. 86-87) support by analogy a late date for this low-level beach. Tests were made by Libby on sheep dung and wood obtained from Danger Cave, near Wendover, Utah, "on old beach of Lake Stansbury, consisting of 2 feet of sand deposited on cemented gravels," and overlain in turn by several feet of later deposits. Both dung and wood were obtained from the sand layer. Their radiocarbon ages were found to be 11,453 ± 600 and 11,151 ± 570 years, respectively. The close agreement of these dates with that of the Mankato maximum in Wisconsin is very striking. Apparently, then, the Stansbury phase of Lake Bonneville (330 feet above

Great Salt Lake) and not the Provo phase (625 feet above Great Salt Lake) is of late-Wisconsin (Mankato) age, and the ages of the higher shorelines will have to be adjusted accordingly. This correlation confirms the tentative one of Flint (1947, p. 473) instead of that of Antevs (1945, pp. 26-30). In accordance with the new correlation the Stansbury and ZX Red Barn beaches represent pluvial maxima of Mankato age and not merely halts in the evaporational decline of deeper lakes.

The multiple pluvial-lake strandlines in south-central Oregon, which now seem older than was supposed ten years ago, are still under scrutiny for further evidence of their ages. As a result of the Danger Cave radiocarbon date, the ages of the Lake Bonneville and Lake Lahontan shorelines need adjustment also. Titles and abstracts in the 1952 program of the Rocky Mountain Section, Geological Society of America, May 8-10, indicate that such re-examinations are in progress. These developments tend to confirm Flint's view that "the concept of a twofold pluvial history is too simple and too rigid to meet the facts" (1947, p. 476).

The interpretation now preferred by the writer is to make the old shorelines older, and the young shorelines younger than heretofore thought possible, and thereby to lengthen a previously telescoped concept of the span of time represented by the pluvial lake history.

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Department of Geology
Oregon State College
Corvallis, Oregon

# ESSAY REVIEW

*Volcanic Geology, Hot Springs, and Geysers of Iceland;* by Tom. F. W. Barth. Carnegie Institution of Washington Publication. 587. Pp. xi, 174; 68 figs., 31 plates. Washington, 1950 (Carnegie Inst. Washington, $5.00, paper bound; $5.50, cloth bound).—With the publication of Barth's monograph, the Carnegie Institution of Washington has made a valuable addition to the contributions its workers have already made to our knowledge of hot springs and related volcanic phenomena. The thermal activity manifested so abundantly in Iceland has long excited attention and been the subject of numerous investigations; in this paper the observations, analyses, and temperature determinations from many scattered sources have been assembled. To them have been added numerous analyses, temperature determinations, determinations of pH in spring waters, and field observations made by the author, so that a survey of thermal activity in Iceland is now available somewhat comparable in scope to the Carnegie Institution publication by Allen and Day on the Hot Springs of the Yellowstone National Park.

The paper is divided into four parts: Part I, Volcanic Geology of Iceland; Part II, Hot Springs; Part III, Geysers; Part IV, Description of Hot Spring Areas. By virtue of this arrangement observations and data are summarized and tabulated, and broad general relationships and conclusions are stated without the reader having been inundated by such a flood of descriptive detail that he is gasping for a breath of unheated vapor, as it were, before any generalizations of wide applicability are drawn. The paper should be complimented for exceptional clarity and simplicity of style, and for abundant illustrations.

Part I is a concise account of the volcanic geology of Iceland and its setting in relation to major crustal lineaments such as the Mid-Atlantic Ridge. It is stressed that present thermal activity is related to a current episode of volcanic activity which started in the Pleistocene and is confined to a broad belt extending northeast-southwest across the island, although numerous hot springs and other manifestations of thermal activity extend beyond the belt of active vulcanism into the early Tertiary basaltic terrane of the Thulean province.

In Part II hot springs are treated in terms of the interplay of groundwater hydrology, subsurface and surface geology, and volcanic activity. The author shows that a combination of conditions more or less peculiar to Iceland lends an overall simplicity to the hydrology of the island. These conditions include a highly permeable and uniform basaltic terrane extending to the base of the geologic column, high rainfall, low runoff and surface evaporation, and

an underlying hydrological substratum of sea water lying at fairly predictable depths below the water table of fresh water. The implication is that because of the relatively simple hydrological and geological conditions, generalizations may be made regarding the fundamental thermal processes with much less probability that the observed phenomena have been influenced by unknown and purely adventitious inhomogeneities in hydrological and geological conditions.

The author finds a strong correlation between the pH of thermal waters and the proximity of the springs to centers of volcanic activity. The acid waters are near active areas and the alkaline waters are more distant. This correlation between pH and the presumed source of thermal energy supplies a unifying thread in the discussion of the genesis and evolution of thermal waters, and, if valid, represents an advance in the classification and correlation of hot spring phenomena over the rather loose chemical-geographical-geological grouping employed by Allen and Day (1935) in the Yellowstone Park study.

Strong support is given the conclusion reached by Allen and Day that volcanic hot springs arise from groundwater to which thermal energy has been added from an underlying magmatic source by the quenching in the groundwater of superheated steam emitted by the crystallizing magma. Following Fenner (1933), Barth reasons that the steam emanating from the magma will be accompanied by other gases and volatiles of a dominantly acid character. With increasing distance from the magmatic source the acids are neutralized by reactions with the rocks through which the thermal waters rise and eventually become alkaline. The evolutionary sequence from acid to alkaline is thus a function of distance from the source of magmatic energy and gas and, by implication, is not necessarily a secular evolution or related to a change in the primary character of the magmatic emanation. This view may be contrasted with the relationships adduced by Allen and Day (1935) who stressed the correlation between the chemical composition (without special reference to hydrogen ion concentration) of thermal waters and the situation in regard to groundwater supply; the "sulfate" waters (generally acid) being found in areas of relatively dry ground where gaseous emanations may be presumed to approach nearer the surface without being absorbed into the groundwater, and the alkaline waters in basins with a deep and abundant circulation of groundwater. The two points of view seem to be easily reconciled, and both may be valid—distance from the magmatic source being of primary importance where groundwater conditions are generally uniform, and relative supply where the groundwater supply is spotty. Barth discounts Graton's (1940) opinion that alkaline solutions rather than acid gases are the primary magmatic emanations, and he disputes Sonder's (1941) conclusion that alka-

line waters are made acid very near the surface by the complete oxidation of $H_2S$, pointing out that alkaline waters typically contain no $H_2S$.

A correlation is drawn between the acidity of spring waters and their proximity to magmatic sources on the one hand and the composition of the gases present in the water on the other. It is concluded that $CO_2$, $H_2S$, and $H_2$ are the only important primary magmatic gases that are discharged *at the surface* in the gaseous state. $N_2$, A, $O_2$, and $CH_4$ are shown to diminish sharply with proximity to volcanic centers and are presumed to be largely introduced by contamination with atmospheric gases and reaction with lignitic beds. Other magmatic gases, above all the halogens, are assumed to be lost quickly by reaction with rock and to appear in the thermal waters only as ions. Barth's conclusions regarding the primary magmatic gases may be contrasted with those of Allen and Day whose calculated primary magmatic gas contains no $H_2$ or $H_2S$ and corresponds in Barth's view to the residual magmatic gas in areas well removed from primary sources; with the view of Jaggar (1940) that nitrogen is an important primary magmatic gas; and with Graton's (1940) opinion that acid gases are not directly derived from magma.

A troublesome fact common to hot spring areas in general is that while HCl is assumed to be an important primary constituent of the magmatic gas that supplies the energy of hot spring waters, and indeed is present in large quantities in fumaroles of direct magmatic origin, $Cl^-$ is present in very low concentrations in acid waters, and, as Barth points out, is 30 to 50 times as abundant in alkaline waters. This appears anomalous if acid gases are in truth the agency of heat transfer between magma and groundwater, and if Barth's hypothesis is correct that acid springs are waters drawn off nearer the magmatic source than alkaline springs. Indeed, without a satisfactory solution to this problem, a great deal of the theory may require modification. Barth's answer (p. 43) is as follows:

> "It is easy, however, to see another reason for the absence of $Cl^-$ and $CO_3^{--}$ from hot-spring water at early stages. Because of their low boiling point, hydrochloric and carbonic acid will, when mixed with sulfuric acid at an elevated temperature, rapidly boil away; therefore, only sulfuric acid will remain in the acid hot-spring waters. The magmatic gases do contain chlorine and carbon dioxide, but as soon as chlorine ions or carbonate radicals enter the ascending acid hot-spring water, they are chased out, or rather rushed ahead into the hot-spring water of later stages, which shows more alkaline reaction and is able to retain the ions. This process is a typical geochemical migration, which may be pictured as a steady stream of chlorine and carbonate ions from the magma, through the gas phase and the

acid hot springs into the alkaline hot springs, and eventually into the sea."

"The flow of this ionic migration has not the same velocity everywhere, but is particularly rapid through the acid hot springs; indeed, so rapid that the concentration at any one time may be almost negligible. In Iceland the average concentration of $Cl^-$ in spring waters with pH below 5 is around 2 parts per million; in New Zealand it is 20. In the alkaline springs of the same regions it is 90 and 600 respectively. Although no great accuracy can be ascribed to these figures, they indicate (since chlorine ions are not consumed by chemical reactions) that the velocity of migration of the chlorine ion in the acid springs is 30 to 50 times greater than in the alkaline ones."

There is a grave question in this reviewer's mind whether Barth has offered any explanation at all in the quoted paragraphs. While it is true that HCl will be volatilized in the presence of strong $H_2SO_4$, it must be recalled that we are dealing here with very dilute solutions. The maximum $SO_4^{--}$ concentration reported by Barth corresponds to that present in somewhat less than a 0.10 normal $H_2SO_4$ solution. Since other positive ions are present, the pH reported for these waters corresponds to an acid concentration of less than 0.01 normal. In very dilute solutions HCl is almost completely dissociated and the vapor pressure of HCl is exceedingly small. Under these conditions HCl is not likely to be volatilized, and to clinch the argument no HCl is reported in any of the gas analyses from any of the acid springs where it should be if it is being boiled out of the water.

The absence of HCl in gas analyses from acid springs seems to be a serious flaw in Barth's argument. Furthermore it tallies with the expectation that HCl is not appreciably volatilized in very dilute solutions owing to its complete ionization. This seems to eliminate the participation of a gas phase in the process of "geochemical migration" by which Cl is rushed through the acid solutions. Failing the participation of a gas phase, Barth has given no clue as to a possible cause of the extraordinary differential in chemical potential of $Cl^-$ in dilute acid as against dilute alkaline solutions. If a diffusion process is involved, why should $Cl^-$ be expected to outrun, as it were, $SO_4^{--}$ by such a wide margin? The ionic conductances of the two ions are almost the same; $Cl^- = 76.34$ and $\frac{1}{2}\ SO_4^{--} = 80$ (Harned and Owen, 1950, p. 172). To be sure, the physical and chemical system represented by a hot spring complex is so complicated under even the simplest geologic conditions that there may well be ways by which the migration of chlorine ions is enormously accelerated in dilute acid solutions, but the process Barth has indicated does not appear promising. It seems appropriate to ask if our present geochemical and geological knowledge of the hot spring process is so complete that the

mere existence of an anomalous concentration gradient in $Cl^-$ is, ipso facto, sufficient to establish it as the typical result of "geochemical migration." Should we not entertain alternative explanations and, since this is a rather critical point, face the implications of the alternatives? Perhaps it is unfair of this reviewer to lay so much stress on a single point in a paper so filled with valuable contributions, but by contrast with the rest of the work, this seems an unfortunate lapse into the kind of geochemical reasoning that appeals to a process for which the only evidence is the effect that it purports to explain.

The relatively uniform basaltic composition of the rocks throughout Iceland allows Barth to make interesting comparisons between the rock alteration effected by acid, neutral, and basic thermal waters. The acid waters tend to cause the replacement of all the original rock minerals by silica with the removal of virtually all of the cations. Neutral waters effect alteration to clays with little removal of alumina and iron. Alkaline waters produce little chemical change, but effect a complete mineralogical reorganization of the rock, mainly into zeolites, chlorite, and calcite. Barth (1948) employs his "standard cell" method of recalculating chemical analyses to a standard number of oxygen atoms to show that the changes are mainly volume-for-volume replacements rather than leaching out of certain components. An interesting contrast is found between the alteration of the Iceland basalts and the Yellowstone Park rhyolites (Fenner, 1936). There is no exchange of potassium for sodium in the Icelandic basalts attacked by alkaline waters as was found in the Yellowstone Park rhyolites. On the contrary, some introduction of sodium takes place. The discussion is supported by a number of new analyses.

Part III, devoted to geysers, gives numerous new and collected observations of the periodicity, temperature distribution, discharge, and other characteristics of Iceland geysers. The role of spring gases in the geyser process is stressed. It is shown that the temperature gradient with depth, wherever measured in geysers, does not attain the boiling point appropriate to the hydrostatic pressure, and tends to be relatively stable during the quiescent period. Therefore Barth believes that ebullition of spring gas is a requisite for the reduction of the hydrostatic head to a point at which boiling can begin at the temperature gradient prevailing. Thus the Bunsen hypothesis of a simple increase in temperature until boiling is initiated at some point in the geyser system is inadequate. The theoretical relations between gas and water discharge, temperature, and depth are discussed, following Thorkelsson (1940) and Versluys (1929). Barth's emphasis on the role of spring gases may be compared with the work of Birch, Graton, and Kennedy (1949)

who found strong fluctuations in temperature in the geyser throat in the region where the temperature gradient approaches the boiling point-depth curve. Barth reports some irregularities in temperature measurements but does not attach critical importance to this phenomena. On the other hand Birch, et al. concluded that there is considerable turbulence at critical depths in the geyser throat, and that the geyser eruption is triggered by a strong, sudden convective overturn bringing hot water from below into a pressure-depth zone where it boils.

Part IV is devoted to detailed descriptions of hot-spring and geyser localities and includes much historical data on past thermal activity as well as the observations of the author.

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MATT S. WALTON, JR.

# REVIEWS

*Time's Arrow and Evolution;* by HAROLD F. BLUM. Pp. xii, 222; 20 figs. Princeton, 1951 (Princeton University Press, $4.00).—It is a rare and significant event to find a new book on so vast a field of speculation, including biological, chemical, physical, geological, and even astronomical considerations, which has been written by an expert. Dr. Blum, who gives the most detailed treatment to biochemical problems, has nevertheless achieved a unified study of the course of evolution from the origin of the earth to the appearance of life as we know it today.

The metaphor, "time's arrow," which was originally applied to the second law of thermodynamics by Sir Arthur Eddington, is somewhat misleading as it appears in the title of this book, although it undoubtedly has esthetic merit. The author affirms his belief that evolutionary processes do not violate the second law, overall changes being in the direction of greater entropy. Nevertheless, the living organism and its environment do not form a closed system and one is left with the impression that entropy considerations are of secondary importance to the problem at hand.

Indeed, the most important and detailed studies in this work proceed along other lines, the author's treatment of entropy being rather casual. In the chapter on "The Fitness of the Environment," he discusses the physical and chemical properties of the substances which are essential to known life forms and shows why one, hydrogen, is unique while others are fitted to support life because of the nature of our planet. After this one finds a general but nevertheless technical description of the chemisty of living systems. Fortunately for the layman, Dr. Blum has preceded this with a lucid introduction to chemical bonding, free energy changes, reaction rates, and chemical equilibria. His discussion leads to some interesting speculation concerning the possible role of the phosphates of adenylic acid as sources of free energy in the formation of the first proteins.

Perhaps of particular interest to the general reader is the interpretation of the effects of reaction rates on the evolution of a complex chemical system. The tendency of the system is to minimize its free energy, but because of the non-dependence of reaction rates upon the free energy changes involved, the system does not undergo a homogeneous degradation. Instead we find a pattern of astonishing complexity being woven—with the appearance of many local anomalies of which life, perhaps, is one. Concerning the oft-remarked improbability of its occurrence, we have the observation that "the longer the time period concerned, the greater chance there is for improbable things to happen."

Dr. Blum's style is delightful and the book is well organized, its scope being larger than it is possible to indicate here.

MAURICE KARNAUGH

*Evolution Emerging. A Survey of Changing Patterns from Primeval Life to Man*, 2 volumes; by WILLIAM KING GREGORY. Vol. I: Pp. xxvi, 736; Vol. II (illustrations): Pp. 1013; 3 folding plates. New York, 1951 (The Macmillan Co., $20.00).—For a third of a century Gregory, through his teaching of advanced students and through his numerous scientific papers, has been the most influential American worker in vertebrate evolution, morphology and paleontology. In this magnificent two-volume work we have an epitome of his lifetime of studies on the vertebrates from fish to man.

Gregory begins by a brief consideration of the various invertebrate groups to give a proper setting for the emergence of the vertebrates. Following a resumé of theories of vertebrate origins, the lower fish groups are discussed and a valuable "who's who" is given of the teleosts, a group which by its complexity is always confusing to the non-specialist. The evolution of amphibians and the radiation of reptiles are given proper consideration; a final third of the text is devoted to the evolution of mammals, with special attention given to the primate family tree leading to man. The specialist in one group or another will undoubtedly disagree with Gregory's conclusions on this or that point, but never has there been put together in one cover so reasonable and comprehensive a picture of vertebrate evolution, here based primarily on osteology and the fossil evidence. Noteworthy is a carefully selected but comprehensive bibliography of nearly 150 pages. One would wish, here and there, that the discussion of novel or disputed points in the story were more fully developed. But we must be willing to sacrifice a few trees (to use the customary metaphor) in order to avoid obscuring the clear paths through the vertebrate forest that Gregory lays out for us.

Unusual is Gregory's scheme of placing the illustrations in a separate volume. Here we have a thousand pages of carefully selected figures. These not merely supplement the text but give in themselves a complete and convincing panorama of the vertebrate evolutionary story.

ALFRED S. ROMER

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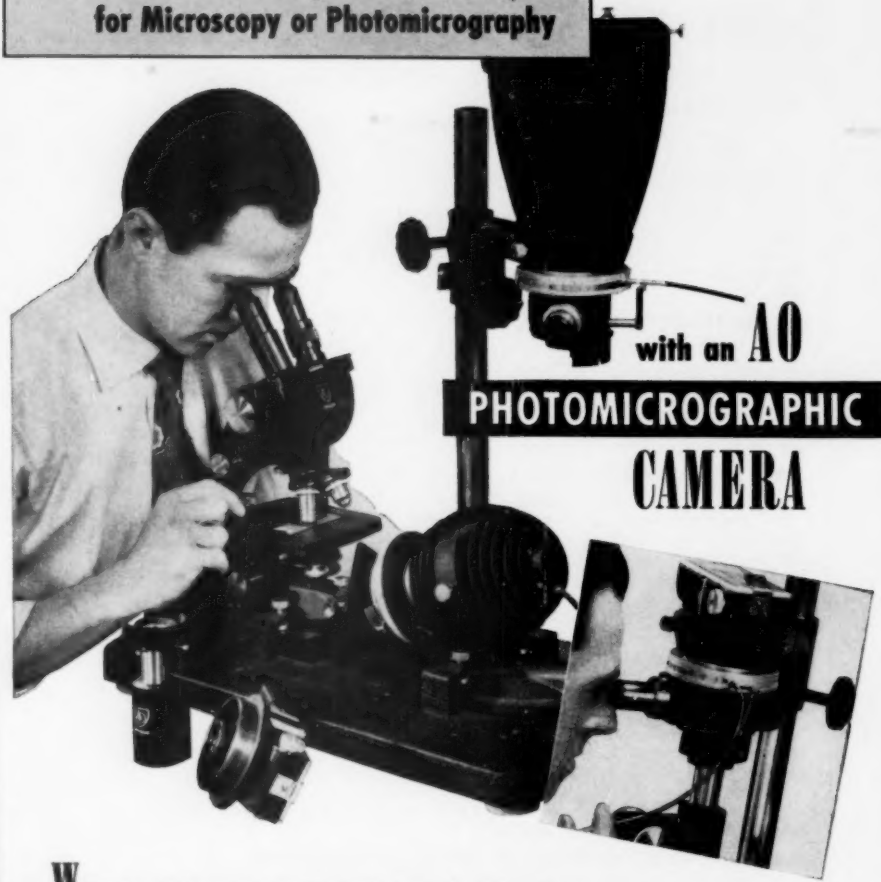

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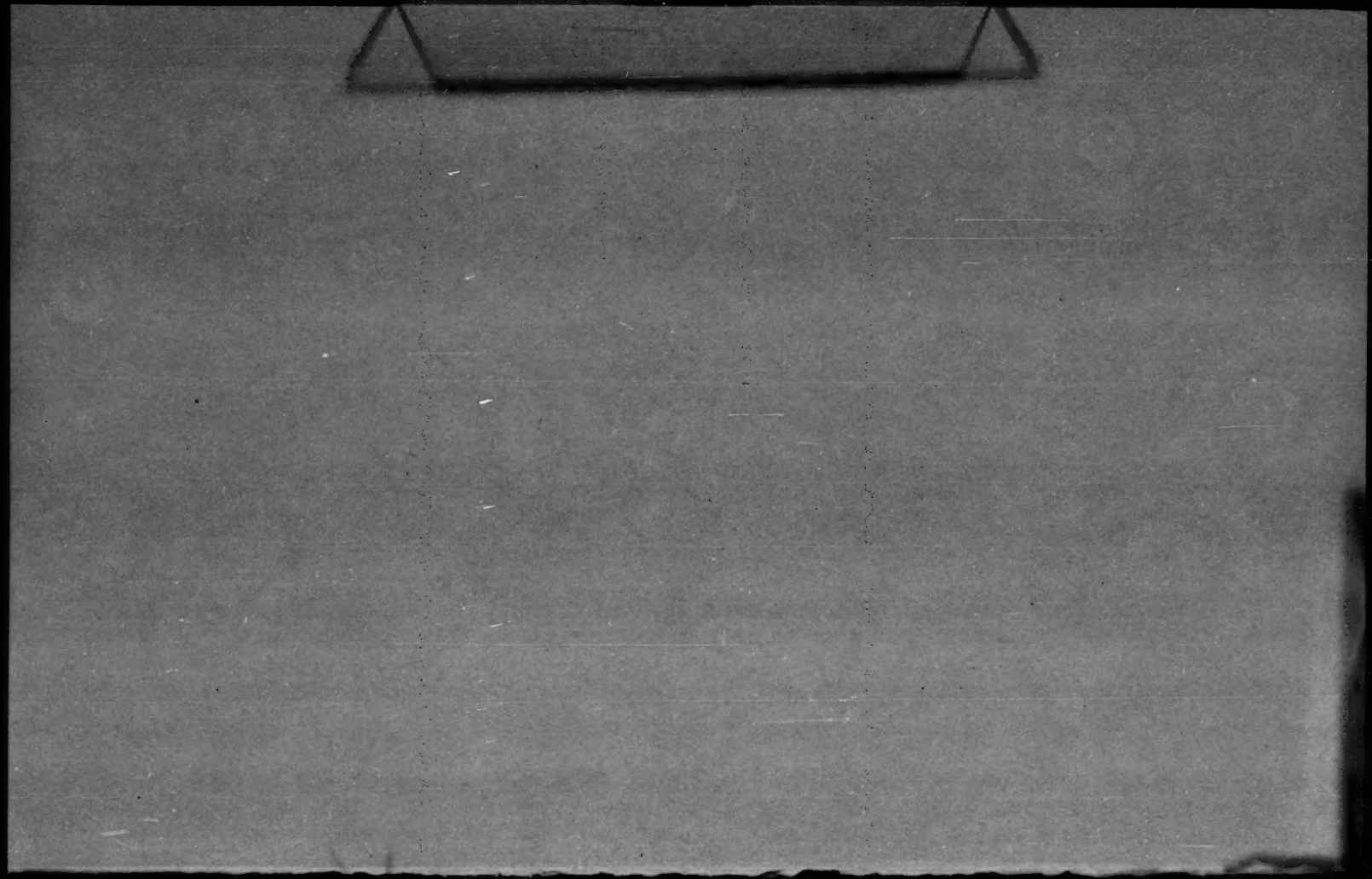

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